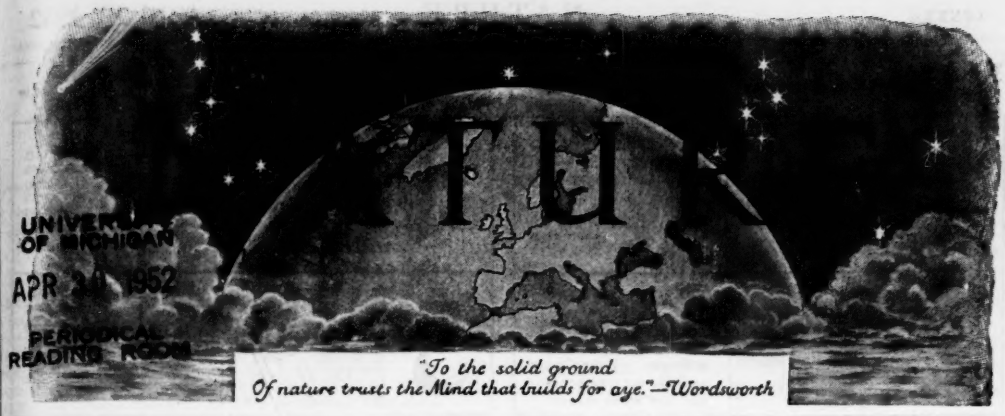

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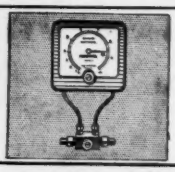

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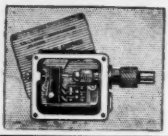

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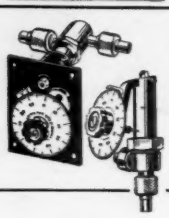

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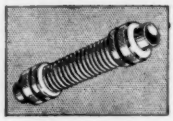

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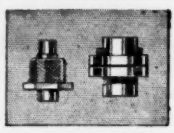

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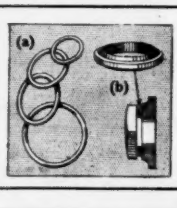

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INFORMATION IN INDUSTRY

SOMEBODY once satirically described the twentieth century as an age of specialization in which the specialist did everything except his job. Certainly it occasions little enough comment that the university teacher should be so bound by the necessity of teaching that his duty to research falls into the background, or that in medicine the clamour for certificates should leave the general practitioner less and less time to diagnose the common cold. Such malfunction by *force majeure* may create little dismay in relation to two professions where achievement is not easily equated with costs. But, as was shown at the conference on "Information Services and Industry", held in London on March 18, even industry, where qualifications misapplied are money and national solvency thrown away, cannot entirely escape the charge.

One of the most fundamentally significant phenomena in industrial development during the first half of the twentieth century has been a steady increase in the amount of scientific and technical information published in official reports, the journals of learned societies and the technical press. Apart from reflecting the rapid strides of scientific progress, its significance lay primarily in the evidence it afforded of a growing willingness on the part of the larger and more progressive organizations to communicate the results of their technical researches, within certain obvious and justifiable limits, to those smaller concerns for which an elaborate research department was beyond immediate reach. Such signs of co-operation, indeed, marked the beginning of the end of the 'cloak and dagger' attitude to industrial research. At the same time, it gave rise to a new problem, namely, the efficient charting of all newly recorded knowledge—and, to solve it, a new science that labours under the name of 'documentation'.

Despite its formal ring, the name no less than the science is to-day part of the life of the major industrial concerns of all highly industrialized societies. Beneath this upper stratum, there ranges in Britain an expanse of companies of intermediate size. Many of them are conscious of the impulse that technical and statistical research already recorded can give to their activities, and not a few make attempts to keep abreast of current information. But this function of industry, as much as any other, should call for efficiency and economy, and it is neither efficient nor economical for chief chemists, design engineers or sales directors to spend half their day reading and recording. The time is already past when all levels of industry, and not only the mammoths, should appreciate that such misuse of specialists is a potent source of waste. In the long run, efficiency and economy are best served by a competent information officer and, be it never so unpretentious, by a properly organized information department.

The conference already referred to, on "Information Services and Industry", which has provoked these comments, was arranged jointly by the Federation of British Industries and Aslib ; its aim was to put

before representatives of firms of intermediate size the case for such internal information departments. Against the background of a scholarly survey by Sir Alfred Egerton of the expansion of technical information services from the first issue of the *Philosophical Transactions of the Royal Society* in 1665 to the abstract bulletins and translations indexes of the present day, Dr. J. H. Chesters (United Steel Companies, Ltd.) and Dr. J. Farquharson (Beecham Research Laboratories, Ltd.) illustrated their papers with examples from their research experience of ways in which the presentation of a piece of information at the right moment and in the right form had led to new products and new processes within their organizations. The direct services which Aslib itself, as the co-ordinating centre in Great Britain for information, provides for industry were described by its director, Mr. Leslie Wilson, and, later, delegates to the conference visited some companies in the London area to inspect modern methods of locating, collecting, storing and disseminating information of every kind.

As this conference proved, Prof. Huntress, the authority on the classification of chemical literature, was right to claim: "We have not been willing to recognize that the necessity for organization, correlation and integration of data of scientific discovery is precisely as important as its original discovery". This is still substantially true, though in recent years the pioneering work of Aslib and of the Royal Society's Conference on Scientific Information in 1948 has combined to create an awareness of the need and, in some important respects, such as subject bibliographies, lists of academic theses, indexes to translated material and so on, to fill in the still wide gaps in recorded knowledge.

Time spent on such specific projects and on the circulation of technical information is, however, largely wasted unless industry itself sets up the appropriate machinery to receive, sift, record, store and apply the information it receives. Even where the idea had taken root in the past, lack of knowledge of the cost often prevented action. In any event, trial and error was, before the Second World War, a process that much of British industry could afford. Now the sands of time have run so low that none can be content to discover and rediscover knowledge which others have already unearthed and recorded. In these circumstances, it is safe to predict that in the next ten years the rate of growth of information services in industry will far outstrip that of the past fifty. It is good news that the Department of Scientific and Industrial Research has provided Aslib with the funds to employ a consultant as a means of encouraging this growth.

One other lesson of the conference should not be forgotten. It is that industry's daily need is not for scientific information alone. A new product is of little use if it cannot be manufactured within the law, transported without deterioration and marketed at a reasonable price. Much therefore needs to be done to chart existing information about world markets, comparative distribution costs, and the legal and sociological aspects of industrial production. By the standards in these fields, the position of scientific and technical information must be considered nearly perfect. But if the truth be told, man has, comparatively speaking, progressed little beyond the laying down of an acceptable theory of information service. The need now is for research into costs and into the refinement of techniques that can make of information service the exact science which it ought to be.

A NEW BRITISH FLORA

Flora of the British Isles
By A. R. Clapham, T. G. Tutin and E. F. Warburg. Pp. liii+1591. (Cambridge: At the University Press, 1952.) 50*s*. net.

THE authors of this Flora claim that their reason for yet another is best explained by a brief historical survey of British Floras; interesting though the survey is, it does not reveal the real reason for the new Flora. The plain reason is that a new British Flora of this standard has been badly needed; in fact, this Flora has been in almost desperate demand for well-nigh seventy years. Other useful new Floras have appeared during that interim; but nothing of such high standard or essential detail or carrying the weight of such authority as this. Since Hooker's "Student's Flora of the British Islands" was last revised in 1884, many advances have been made in plant taxonomy and systematics and in plant ecology and genetics, and a host of changes in botanical nomenclature has consequently occurred (not always justifiable if botanists could be persuaded at times to bring a sense of proportion to bear and realize to the full the implications and endless ramifications which such changes incur).

At last, however, the botanist can feel satisfied. As Sir Arthur Tansley states in his foreword, a "new British Flora has been a desideratum for the past half century and urgently needed during the last thirty years". (An indication of the excessive length of time that this book has been going through the press is given by reference to Sir Arthur as "Professor A. G. Tansley".) The Flora has been more than "urgently needed"; its absence was causing real mischief among fully fledged botanists and students alike. With Sir Arthur's hint, however, that the new Flora is not primarily for specialists (also claimed by the authors) we do not entirely agree. It is clear that students of the subject must have this Flora; but specialists too, even of taxonomy and systematics, will find it indispensable if only for reference to families outside their own specialism. In fact, all professional and amateur botanists and students of botany (at any rate in Britain) will scarcely be able to manage without it. They did before it was published; but then we were always out of date. Now, at any rate for a time, that excuse cannot be made.

The compilation of the subject material must have been an almost Herculean task, in spite of the fact that there were three authors who also acknowledge the assistance of a number of other botanists. Naturally, such a Flora does not go into all the details of critical genera, neither are all varieties named. Yet all the families represented either indigenously or by introduced forms in Britain of the Pteridophyta, Gymnospermæ and Angiospermæ are

dealt with. Apart from the fact that the three authors are well-recognized specialists, it is significant that nearly all the thousands of descriptions have been based on direct observation of fresh or herbarium material. The keys to identification make use of those plant characters which are most easily recognized either in the field or in the herbarium. For some of the larger families, synopses of classification are given; in others, the tribes are described. We are particularly grateful that the authors decided to drop all initial capital letters for certain specific names and use the small initial letter throughout. The use of the capital for those specific names which are those of other genera or of botanists or are nouns or the pre-Linnean names for the plants, though recommended by the International Rules of Nomenclature, always appeared to be unnecessary and confusing pedantry—giving many more chances to students and authors to make mistakes, taxing the memory unnecessarily and teaching practically nothing.

Apart from keys to identification, the Flora describes each family fairly fully, giving habits and habitats, vegetative structure and floral details. Each genus to be found in the wild state (native or introduced) in Britain is also similarly described. Then for each plant there is a description giving the present recognized scientific name, former scientific names, structure of flower, time of flowering, chromosome number (where known), habit—in the rarer or more localized cases even giving the areas.

There are about eighty line diagrams which serve their main purpose in aiding identification; but could have gone still further and illuminated the text had they been drawn with more care and æsthetic appreciation—not to be ignored even in a book of this sort.

Botanists now have what they have been wanting for a very long time—a safe foundation for their study and work. It is to be hoped that unless our specialists find something very seriously in error in this first edition or eventually develop a genus or family to an extent warranting fundamental changes, this book will remain our authority for the next fifty years, allowing for new editions involving reasonable changes. Then the student (a potential specialist) and the specialist himself can go on their way with confidence, and—an important point probably ignored by botanists—other intrepid but less-specialized authors and above all harassed editors of scientific journals can rest assured that with their "Clapham, Tutin and Warburg" on their reference shelves, the manuscripts they either write or edit are not out of date at any rate before passing through the printer's hands. Moreover, those curators of public parks and gardens who conscientiously endeavour to keep their labelling (if any) of their plants up to date have been getting little peace. Perhaps they obtained some relief from the fact that horticultural varieties are usually beyond the botanical pale. This book will be a boon to them also, in spite of the fact that some of their plants are exotics. In this connexion, it is at least amusing to quote from an article entitled "Plant Names: a Botanical Nightmare" reprinted, by courtesy of the Garden Club of America, in the recent issue of the *Journal of the Royal Horticultural Society* (77, March 1952). A professor of the history of the English language took a friend to the New York Botanical Garden to see his favourite shrub. "He explained that it was the Japanese Quince and that its botanical name was *Pyrus japonica*. His friend looked at the label, gave him a contemptuous look, and informed him that he was mistaken, that the name was *Cydonia lagenaria*. He was quite upset about this. . . . It was still more disconcerting to come the next year with other friends, tell them the story, explain it was now called *Cydonia lagenaria*, have them look at the label and again inform him that he was wrong, that it was *Cydonia japonica*. The third year he took no chances. He went alone and looked at the label. Sure enough, it had still another name, *Chænomeles lagenaria*."

Later on, the professor was searching in a library for the "derivative of the word 'chair', and discovered in an old book that it came from the Chinese word 'cha'. . . . But in the book was a diagram to show exactly what the 'cha' meant, and extraordinarily enough, it turned out to be a table, while on the next page the word 'ta' from which 'table' had been taken, had a beautifully illustrated drawing showing a chair. [He] thought that hereafter chairs should be called 'tables' and tables should be called 'chairs'. This would be an inconvenience to those who were trying to sell dining-room chairs, to remember whether the word chair would mean something to put the dinner on or something to sit on. . . . [Nevertheless, thinking of the botanists, he] decided to take the matter up with the head of his University the next day to have this change made".

But it was just a dream, and the professor was "relieved therefore to feel a hand on his shoulder shaking him and to hear his wife's voice telling him to wake up, put out the cat (which he vaguely remembered ought to be called an ostrich) and come upstairs to bed".

The three authors of this "Flora of the British Isles" are not only to be congratulated on an important piece of work well done, but also to be thanked for the great favour they have bestowed on British botany. In spite of the fact that all costs connected with printing and publishing have soared to very high levels, it seems a pity that the Syndics of the Cambridge University Press could not have seen their way to setting a lower price on the book, especially in view of the fact that it will become an essential part of the equipment of all botanists and students of botany (to be possessed, not borrowed), and therefore ought to command large sales.

L. J. F. Brimble

FRANCIS BACON'S PLACE IN SCIENCE

Francis Bacon, Philosopher of Industrial Science
By Benjamin Farrington. Pp. viii+198. (London: Lawrence and Wishart, Ltd., 1951.) 12*s.* 6*d.*

WHILE Francis Bacon has passed into history as a reformer of logic, he should be remembered, says Prof. B. Farrington, rather as a reformer of society. The fault lies with his early editors who, instead of presenting the "Novum Organum" as a part of "The Great Instauration", gave it first place among his philosophical works: and thus "The ringing challenge of *The Great Instauration* is muffled under the academic cloak of the *Novum Organum*". Bacon was, in fact, advocating a total renovation of human society: the new method, the "Novum Organum", was almost incidental, the description of the weapon to be employed in the great business of

the recovery of man's dominion over Nature by the discovery of her secrets and her mechanism and their application for the relief of man's estate.

Emphasis on the neglect in appreciating Bacon's great aim goes, perhaps, a little too far, because Bacon has suffered little loss of reputation in the treatment accorded to his memory by historians of science, who have not failed to give him full credit for his vision of Solomon's House, which inspired the founders of the Royal Society, and to recall his brilliant advocacy of the application of scientific knowledge, which gave to the Society in its early years so much of its practical interests and empirical outlook. Happily, the historians of science cannot be called to account for what the philosophers and the historians of philosophy have said of one whom Prof. Farrington rightly calls "this very original and unorthodox figure", but a sounder and integrated judgment is here presented on Bacon's whole work.

Bacon wrote following upon an age that had known the writings of Biringuccio, Agricola and Palissy, and had witnessed remarkable developments in mining and metallurgy and the rise of a printed technological literature; and thus, almost from his boyhood, he was inspired with a contempt for Plato and Aristotle and Scholasticism and with the great vision of an alliance between, on one hand, the 'new philosophy', that is, the study of Nature by experiments, and, on the other, what we would now call technology, by which alliance the material conditions of life would be improved and transformed. So far as his new method goes, none can now be found to praise him; for no man ever discovered anything by the working of that cumbersome logical mill. Yet the great vision that makes him one with the moderns remains and is possibly being at last realized in our time. The development of Bacon's thought seen against the background of his times, together with the details of his interesting life and of his many works, are well summarized in this well-written short study. A reprint of Bacon's description of "Solomon's House" from the "New Atlantis" is appended. D. McKie

RECENT DEVELOPMENTS IN PROTEIN CHEMISTRY

Advances in Protein Chemistry

Edited by M. L. Anson, John T. Edsall and Kenneth Bailey. Vol. 6, with Cumulative Subject Index for Vols. 1–5. Pp. xi+549. (New York: Academic Press, Inc., 1951.) 9.50 dollars.

THIS sixth volume in a valuable series will be of particular interest to physical biochemists (or chemical biophysicists—what is in a name?) because of two splendid articles. The first, by R. W. G. Wyckoff, is concerned with the electron microscopy of biological macromolecules, and in it are reproduced a number of the beautiful micrographs we expect from the author. It is fascinating to see the beginnings of a new subject, for molecular cytology is certainly that. Wyckoff's own statement is strangely moving and sticks in one's memory: "It is impossible at this time to perceive more than the broadest outlines of this visual chemistry or even to be sure of the directions in which it will most actively develop, but that such a chemistry will inevitably arise and that it will yield exciting new prospects over the way living matter functions is certain beyond doubt".

A complementary method of studying large molecules is afforded by the measurement of the intensity of light scattered by their solutions at different angles, and the molecular weights of more than twenty proteins have now been determined by this means. The other noteworthy article is that on "Light Scattering in Protein Solutions", by P. Doty and J. T. Edsall, which covers much more ground than its title suggests. It embraces a discussion of the fundamental theory of the scattering of light by liquids and by systems of two or more components, as well as a useful assessment of experimental techniques. The method is particularly suited to the study of the reactions which proteins undergo in solution, and considerable advances in this field may be expected in the future. This scholarly and stimulating article must be considered indispensable reading for the physical biochemist.

Much may be learnt about the structure of proteins from the consideration of the simpler polypeptides that can be synthesized artificially, and the account by E. Katchalski of the poly-α-amino-acids assembles a great deal of useful information about these interesting compounds.

The remaining five contributions are of a more biochemical, or even clinical, character. Egg proteins are reviewed by H. L. Fevold, and iodoproteins, both natural and artificial, by J. Roche and R. Michel. There can be very little work of interest that has been omitted from mention in these two comprehensive summaries. An unusual feature of the article by J. Bjorksten on "Cross Linkages in Protein Chemistry" is the large number, 278 in all, of descriptive references to patents.

Considerable interest is now centred on the role of a particular amino-acid, glutamic acid, in normal and abnormal metabolism of the brain, and the review "Glutamic Acid and Cerebral Function", by H. Waelsch, is therefore timely. Finally, there is the very full account by H. Pollack and S. L. Halpern of "The Relation of Protein Metabolism to Disease" which, it is to be hoped, will be read and acted upon by the medical advisers to the Ministry of Health and, perhaps what is more to the point, to the Ministry of Food. D. P. Riley

PHYSICS OF CREEP IN METALS

Creep of Metals

By L. A. Rotherham. (Physics in Industry Series.) Pp. 80+2 plates. (London: Institute of Physics, 1951.) 15*s*. net.

THE earliest observations on the creep of metals were generally made by physicists. The study was not energetically pursued until it became technically important, when it was taken up by metallurgists and engineers endeavouring to produce alloys more resistant to creep. They employed empirical methods and were gradually successful. However, developments by the empirical method are laborious, and to-day one seeks for guidance from the theory of the plastic deformation of metals that has been developed during the past twenty years. In this search physicists have joined. The wheel has now come full cycle with the publication of this book dealing with creep, which is written by a physicist—albeit one with first-hand experience of technical creep—and published by the Institute of Physics.

A book from such a quarter might be expected to have as its aim the explanation of the physical processes that take place during, and constitute, creep deformation. However, knowledge has not reached the stage where this is fully possible. The most one can hope for is a statement and discussion of the present position, and this is what the book achieves. It does this in a non-mathematical way, on the whole, although some mathematics is used in the chapters on transient and steady-state creep.

The chapters are: (1) "Characteristics of the Creep Curve", which discusses the different types of curve and gives empirical formulæ; (2) "Crystalline Flow", which describes one of the best-understood deformation processes and is accordingly one of the most specific chapters; (3) "Metallographic Features of Creep", which deals principally with the surface manifestations of creep in aluminium; (4) "Grain Boundary Creep", largely devoted to a review of the work of T. S. Ke on small grain-boundary displacements; (5) "Transient Creep", which discusses the current theories of transient creep in the light of knowledge of actual creep curves; (6) "Steady State Creep", which is mainly a review and a critical discussion of theories that have been put forward and recounts the few experiments that have been done with the aim of testing these theories; (7) "Tertiary Creep"; and (8) "The Development of Creep Resistant Alloys", which deals with the development during the past decade of alloys suitable for service at very high temperatures.

The information contained in the first seven chapters did not contribute much to this development compared with the help that a strong, coherent theory of creep could give. On the last page is given a programme of research to acquire the experimental material necessary to develop such a theory. One suspects that, if this is to be accomplished in a reasonable time, the proportion—probably small at present—of research workers engaged on this kind of work will have to be considerably increased.

Some misprints have been noticed: "eqn. 28" on p. 47 should be "eqn. 36"; the term "c" is omitted from eqn. 40 on p. 48; eqn. 54 on p. 55 should include the term "$Y6$" in place of "YS" to be consistent with eqn. 53. Sampling the index showed it to be fairly accurate; the subject was at least always mentioned on the page indicated.

The book seems dear at 15*s.* for seventy-six pages. However, its quality is such that it will be used by most people having to deal with creep. It is suitable not only for physicists but will also be equally useful to metallurgists and engineers concerned with creep.

ALGEBRAIC FUNCTIONS

Introduction to the Theory of Algebraic Functions of One Variable

By Claude Chevalley. (Mathematical Surveys, No. 6.) Pp. xi+188. (New York: American Mathematical Society, 1951.) 4 dollars.

"ALL specialized research," says Schrödinger[1], "has real value only in the context of the integrated totality of knowledge." Prof. C. Chevalley's book is an account of specialized research which is so important that the review in the *Bulletin of the American Mathematical Society*[2] extends to nearly fourteen pages. On the other hand, the subject is treated in a one-sided manner which may support the pessimist who feared that some day no two mathematicians will be able to understand each other. Weyl remarked that "the angel of topology and the devil of abstract algebra fight for the soul of each individual domain". Some consider that two words in this pronouncement should be interchanged, but no one can doubt that the second antagonist, however he may be described, has gained a complete victory in this book.

In his introduction, Prof. Chevalley gives a defence of his treatment. He is, of course, well aware that the theory of algebraic functions is closely allied to that of the algebraic geometry of curves. This alliance is so close that many consider the geometric treatment of algebraic functions to be the natural method. But there is a second method, that of abstract algebra, which emphasizes the analogies with algebraic numbers. Unfortunately the partisans of either point of view have what the author describes as "strong emotional reactions". He does not see any virtue in a synthesis, and says, "without attacking in any way the validity *per se* of the geometric approach, we have not tried to hide our partiality to the algebraic attitude, which has been ours in writing this book". He seems to imply that the geometric and the algebraic methods are the only ones available, but some consider that a third method, that of the theory of functions, used by Riemann himself, is still in many ways the most powerful of all.

Chapter 1 is entitled "Places and Divisors". Instead of starting with complex numbers, Prof. Chevalley starts with the more general concept of a field. This has the advantage (which is not mentioned) that the greater part of Chapters 1, 3 and 4 applies with little change to number-fields. Chapter 2 deals with the genus and the theorem of Riemann–Roch, which is proved very briefly by regarding differentials from an unusual point of view (to be proved equivalent to the usual one in a later chapter). Chapter 3 introduces the p-adic completions of the function fields. Chapter 4 deals with extensions of fields of algebraic functions of one variable. In Chapter 5, which deals with extensions of fields of constants, some may think that the algebraic method shows its weakness, as it has great difficulty in proving properties that are easily dealt with geometrically. Chapter 6 returns to differentials, which have already been used in some earlier chapters. Some of the questions discussed in Chapters 4, 5 and 6 have never been treated before. Chapter 7, much longer than any of the other chapters, deals with the Riemann surface, not from the usual point of view (which the author describes as the "scissor and glue" method), but by an abstract method for which the author claims "we have avoided the cumbersome decomposition of the Riemann surface into triangles". This chapter contains some interesting and original features, but whether it is an improvement on the usual treatment is open to doubt. The book concludes with an index and a short bibliography.

The author's treatment, it may be conceded, appears to be the best yet known for the study of certain parts of the subject, and has the merit ot pointing out opportunities for further research. If may therefore be commended to the advanced student, with the proviso that he should certainly seek elsewhere a knowledge of other aspects of the subject, so that he can see the relation of the specialized approach to the general body of mathematics as a whole.

H. T. H. PIAGGIO

[1] "Science and Humanism" (Cambridge University Press).
[2] *Bull. Amer. Math. Soc.*, 384 (September 1951).

Symposium on Cytology

By William L. Doyle, Hans Ris, Franz Schrader and C. Leonard Huskins. Pp. viii+69. (East Lansing, Mich.: Michigan State College Press, 1951.) 2 dollars.

THE four papers in this symposium show four methods of discussing cytology. W. L. Doyle, with a good introduction on cell enzymes, presents his own study of phosphatases in the cells of the appendix and kidney of the rabbit. Hans Ris reviews recent work on chromosome and nuclear chemistry. He points out that the contents of samples of nuclei have been shown to be constant in respect of deoxyribose nucleic acid, and he concludes, perhaps rashly, that individual chromosomes do not vary in this respect in the mitotic cycle. Franz Schrader discusses, concisely, some differences of detail between the tactoid and protein chain hypotheses of spindle structure. He works on the assumption that they are mutually exclusive. C. L. Huskins winds up the proceedings with a pastoral discourse on the relations of "Science, Cytology and Society" combined with some observations of his own on meiosis (not mitosis) in the roots of onions. C. D. Darlington

The Classical Theory of Fields

Translated from the Russian by Morton Hamermesh. (Addison-Wesley Physics Series.) By L. Landau and E. Lifshitz. Pp. ix+354. (Cambridge, Mass.: Addison-Wesley Press, Inc., 1950.) 7.50 dollars.

THIS is an admirable systematic account of the theory of electromagnetic and gravitational fields, which is part of a general course in classical physics. The English-speaking reader will perhaps be surprised at the omission of all references to the theory of dielectrics and of magnetic materials, but these subjects are probably discussed in another book of this series. On the other hand, he will be pleased at the prominence which is given to the special theory of relativity, which is indeed the basis of the presentation.

This treatise gives a general account of relativistic mechanics, including, of course, the motion of point charges in electromagnetic fields. Maxwell's equations are studied in detail, and are applied to steady fields, electromagnetic waves, problems of physical optics, the field of moving charges, and the general theory of the radiation of electromagnetic waves. The last two paragraphs introduce the reader to Einstein's general theory of relativity, and discuss the motion of a particle in a gravitational field, and the formation of Einstein's gravitational field equations.

The book is beautifully printed and produced, and has a good index.

An Introduction to Applied Mathematics

By Prof. J. C. Jaeger. Pp. xiii+446. (Oxford: Clarendon Press; London: Oxford University Press, 1951.) 35*s*. net.

ALL those who are called upon to teach mathematics to students of physics and engineering will feel themselves under a great obligation to Prof. J. C. Jaeger for this most useful and timely volume. The author gives an admirable survey of many, if not all, of those topics in applied mathematics which are needed by the physicist and engineer. The work covers, of course, the subject of ordinary and partial differential equations, with special reference to boundary-value problems and numerical methods. There is a most useful chapter on electrical circuit theory together with an introduction to the subject of Fourier series and integrals. Vectors, particle dynamics, rigid dynamics and Lagrange's equations each receive one chapter. There are numerous useful examples, and the whole volume is clearly written by a master of mathematical exposition.

There is perhaps just one criticism which might be levelled, and that is that no proof whatever is given of the convergence of Fourier series or integrals to the generating function. It would be most valuable to students if an elementary proof were given, even though it applied to a very restricted class of functions.

Linear Polymers

By Elizabeth M. Frith and R. F. Tuckett. Pp. xi+355. (London, New York and Toronto: Longmans, Green and Co., Ltd., 1951.) 18*s*. net.

THIS treatment of a rapidly growing subject is well thought out, and the authors are to be congratulated on a fine piece of work. Every section bears the unmistakable stamp of careful preparation; the preliminary marshalling of material, its digestion and thoughtful re-presentation are everywhere evident. The book is, for this reason, personal and holds the attention in spite of the high concentration of facts and theories which are to be found in it. There is, indeed, a great deal of information, critically presented, in this book. It can be warmly recommended to all research workers interested in the fundamental physical chemistry of high-polymeric systems and is especially suitable for those just entering the field.

The Topology of Fibre Bundles

By Norman Steenrod. (Princeton Mathematical Series, No. 14.) Pp. viii+224. (Princeton, N.J.: Princeton University Press; London: Oxford University Press, 1951.) 32*s*. 6*d*. net.

THIS book is the first to give an account of the theory of fibre bundles, a subject which was initiated during the years 1935–40. The work of Whitney, Hopf and Stiefel indicated the importance of the subject for applications of topology to differential geometry, in particular to the study of properties 'in the large' of differentiable manifolds. The subject includes also a topological study of the coset decompositions of Lie groups by subgroups, and the theory of covering spaces used in analysis.

The author has eliminated a certain amount of confusion which had existed owing to different definitions of 'fibre bundle' being in use, as it was not clear previously that any one definition would suffice for all requirements.

The book is divided into three parts: (1) the general theory of bundles; (2) the homotopy theory of bundles; (3) the cohomology theory of bundles.

Part 1 closes with sections on covering spaces and the fundamental group. Part 2 opens with a survey of the homotopy groups of Hurewicz, since no treatment of these had previously appeared in book form. Part 3 includes a survey of cohomology theory, since the standard treatments do not contain the generalized form required in the text.

The book is 'difficult' in the sense that considerable previous knowledge of algebraic and general topology is assumed on the part of the reader. It is, however, ably written, and is well worth a close study by algebraic topologists. The author has himself made a substantial contribution to the subject.

R. G. Cooke

THE DEEPEST OCEANIC SOUNDING

By Dr. J. N. CARRUTHERS and Commander A. L. LAWFORD, R.N.(retd.)
Oceanographical Branch, Hydrographic Department, Admiralty

THE recent announcement in the press that H.M. survey ship *Challenger* had made what is claimed to be the deepest oceanic sounding yet recorded will have aroused the interest of those who are better able than the general public to judge the validity of such a claim. In what follows, therefore, the steps taken during the course of the sounding operation to ensure accuracy will be described, and the actual depth which can reasonably be claimed will be assessed.

On June 14, 1951, H.M.S. *Challenger* (Commander G. S. Ritchie) was crossing the southern extremity of the Mariana Trench, some 200 miles to the south-west of Guam, using a supersonic method of sounding. Bottom was lost at 4,100 fathoms (7,498 metres) and thereafter low-frequency echo soundings were obtained by exploding a small charge beneath the surface and picking up the echo from the bottom on a hydrophone, the time interval being recorded by camera. In the vicinity of lat. 11° 20′ N., long. 142° 16′ E., soundings were obtained which approached 6,000 fathoms (more than 10,000 metres).

Later in the day, a determined attempt was made to check these deep soundings by wire. To achieve this, a 140-lb. iron weight was secured to the end of the wire of the taut-wire machine and led over the stern roller. An elliptical float was secured to the weight to reduce the strain on the wire during the early stages of descent; it was expected that the float would implode and have no further effect at several hundred feet. The ship was held stern to wind, which was light, and by going slow astern from time to time the slope of the wire was kept to a minimum. While the direction of drift was being ascertained, the slope exceeded 5°, but after reaching 2,000 fathoms (3,658 metres) it was never allowed to exceed 3°. During the operation the brake pressure on the taut wire machine was gradually increased.

The cast was commenced at 5.10 p.m., and at 6.40 p.m. the bottom was struck. In the commanding officer's words, "the strike was spectacular, the wire ceasing to run out quite suddenly". The dial reading of the taut-wire machine was 5·863 nautical miles at the moment of striking. As no calibration figures were available for the machine, the circumference of the counter wheel was checked by direct measurement and by measuring the length of wire run off per revolution. The corrected depth obtained by applying the results of these measurements to the dial reading was 5,954 fathoms (10,888 metres).

Low-frequency echo soundings were obtained both before and after the wire sounding. After making due allowance for the error of the camera timing-system, the mean sounding velocity for the depth in question, the depths of the charge and hydrophone, the accuracy of reading the times of arrival of reflected sounds and the guaranteed accuracy of the radio time-signal used for checking the camera speed, these soundings were found to be 5,663 fathoms (10,356 metres) and 5,637 fathoms (10,308 metres) respectively, with a possible error of ± 17 fathoms (31 metres).

Reverting to the wire sounding, a correction must of course be applied to allow for the slant of 3°–5°. For the length of wire out, a deduction must be made[1] of 33 fathoms (60 metres) for angles between zero and 4°. However, Commander Ritchie noted that structural hindrances made it difficult to read the slant accurately, and it would therefore be safer to make a deduction of 55 fathoms (100 metres). Furthermore, allowance must also be made for slip of the wire on the taut-wire machine, and for stretch of the wire due to its own weight and that of the sinker at the moment of striking bottom; this weight amounted to some 2 cwt. (100 kgm.) in water. It is known that the Dutch vessel *Willebrord Snellius*, when sounding in a similar depth of water, hove in 37 metres more wire than she had apparently paid out. Again, the German *Meteor* hove in about 16·6 metres excess wire in waters of half the depth of those where the *Challenger* was operating. These figures agree with each other very well, and it therefore seems reasonable to assess the slip in *Challenger* as 35 metres. If stretch be included, it is suggested that the total correction necessary under these two heads is in the region of 27 fathoms (50 metres). Applying both corrections discussed above to the reported sounding, the depth now arrived at is 5,872 fathoms (10,738 metres).

It would, of course, have clinched the matter if the depth could have been checked by thermometric sounding. Unfortunately, available unprotected thermometers would not serve for more than half the depth concerned. Pinke[2] gives the greatest depth measured thermometrically on the *Snellius* expedition as 3,451 fathoms (6,313 metres). It is possible, however, by using the 'stiffest' unprotected thermometer, to check depths down to about 4,372 fathoms (8,000 metres).

Richter and Wiese, the famous German makers of deep-sea thermometers, have stated that they can

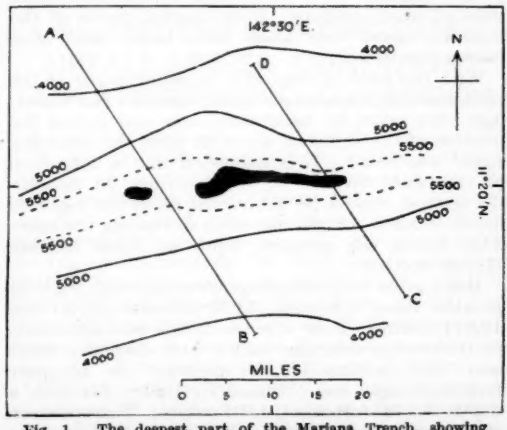

Fig. 1. The deepest part of the Mariana Trench, showing *Challenger's* two runs across it (*A–B*, *C–D*). The solid black portion represents depths greater than 5,900 fathoms

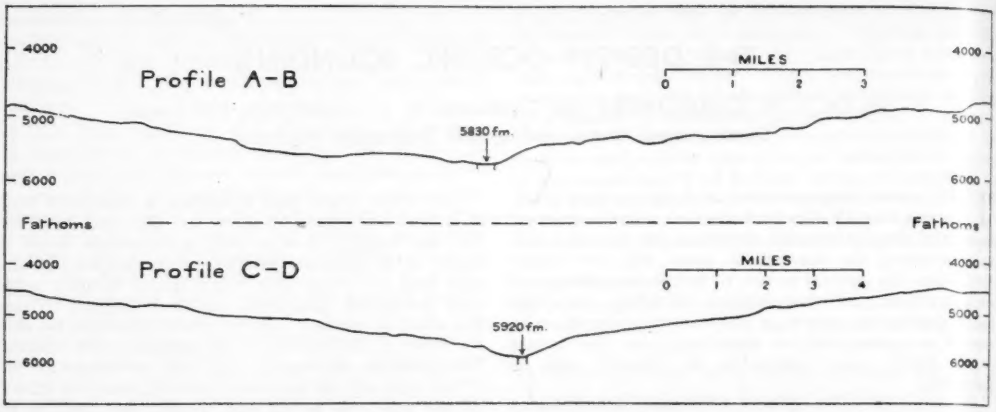

Fig. 2. The central portions of the two profiles across the Mariana Trench (true scale)

probably produce an unprotected thermometer scaled to about 100° C., instead of the usual 60° C., to cater for very great depths. Even so, the scale of such an instrument would be only half as open as in the present thermometer, since the overall length must remain the same if existing frames are to be used. Although both frame and thermometer could be lengthened, risk of breakage would be greatly increased thereby.

While this article was being written, a second communication was received from *Challenger* reporting that a return visit had been paid to the Mariana Trench towards the end of October, when even greater depths had been found. Prior to this visit, the supersonic equipment had been overhauled and modified to permit echo soundings to be obtained down to the greatest depths encountered.

Two closely sounded profiles were run across the Trench (Fig. 1), and in position lat. 11° 19′ N., long. 142° 15′ E. (about 1·7 miles south-east of the earlier wire sounding), a greatest depth of 5,960 fathoms (10,900 metres) was reached.

Since this sounding was obtained by supersonic equipment, very little correction, if any, is necessary on the score of non-vertical reflexions. As will be seen from the profiles (Fig. 2), the deepest part of the Trench appears to be more than a mile wide, with sides of small gradient. Very careful checks of the machine speed were made both before and after taking this sounding.

With the method used, the recording paper of the echo-sounding apparatus was not marked; but soundings were taken by using earphones and noting the position of the stylus on the scale when the returning signal was heard. It is suggested that a correction of one scale division of the recorder, 20 fathoms (37 metres), should be subtracted from the reported depth to allow for possible error in reading the scale. This leaves the greatest depth at 5,940 fathoms (10,863 metres).

Many other deep soundings were reported, the next greatest being 5,950 and 5,930 fathoms (10,881 and 10,844 metres). Four wire soundings were also made by the method described earlier; the deepest of these was 5,922 fathoms (10,830 metres). An adequate bottom sample was obtained by Bailey rod from a depth of 5,744 fathoms (10,504 metres). This specimen is now in the custody of the Mineralogical Department of the British Museum (Natural History), whence has come the information that it consists of brown-coloured abyssal ooze with Radiolaria and diatoms, having negligible calcium carbonate content but containing traces of volcanic dust. A water sample was collected from 5,670 fathoms (10,369 metres). (All depths in this paragraph are given as reported.)

The greatest depths of the oceans reached by echo and wire soundings have been listed by Wüst[3]. This work, by a great authority on the subject, conveniently post-dates the paper[4] by Hess and Buell, and therefore includes the careful assessments by these two American writers of the Cape Johnson Depth, until now claimed to be the greatest recorded. The six greatest depths listed by Wüst are, in descending order of magnitude:

(1) *Cape Johnson Depth* (Philippines Trench): lat. 10° 27′ N., long. 126° 39·5′ E., 10,497 metres = 5,740 fathoms. Assessed possible error, ± 91 metres. High-frequency echo sounding.

(2) *Emden Depth* (Philippines Trench): lat. 9° 42·1′ N., long. 126° 52·1′ E., 10,400 metres = 5,686 fathoms. Assessed possible error, ± 140 metres. Audio-frequency echo sounding.

(3) *Ramapo Depth* (Japan Trench): lat. 30° 30′ N., long. 142° 30′ E., 10,374 metres = 5,673 fathoms. Assessed possible error, ± 145 metres. Audio-frequency echo sounding.

(4) *Snellius Depth* (Philippines Trench): lat. 9° 42·4′ N., long. 126° 51·2′ E. and lat. 9° 39·0′ N., long. 126° 51·0′ E., 10,130 metres = 5,539 fathoms. Assessed possible error, ± 100 metres. Audio-frequency echo sounding.

(5) *Snellius Depth* (Philippines Trench): lat. 9° 40·7′ N., long. 126° 51·3′ E., 10,068 metres = 5,505 fathoms. Assessed possible error, ± 100 metres. Wire sounding.

(6) *Planet Depth* (Philippines Trench: lat. 9° 56′ N., long. 126° 50′ E., 9,788 metres = 5,352 fathoms. Assessed possible error, ± 95 metres. Wire sounding.

It must not be forgotten that the Royal Danish Vessel *Galathea*, which carries a very deep-reaching echo-sounding set, has been operating in the neighbourhood of the Philippines Trench. So far as is known at the time of writing, the greatest depth she has obtained is 5,763 fathoms (10,540 metres) in position lat. 10° 23·8′ N., long. 126° 40·5′ E. This is 23 fathoms (43 metres) greater than that found by *Cape Johnson*. However, the greatest depth found

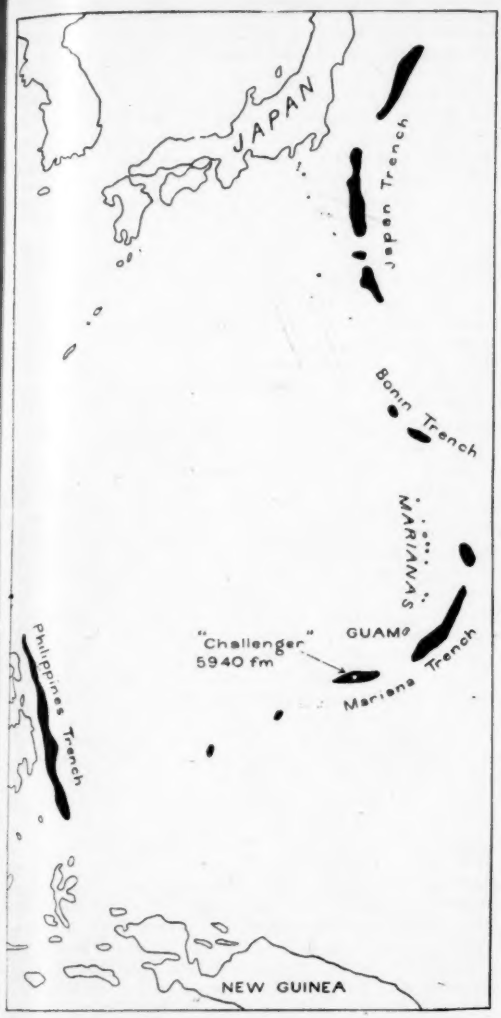

Fig. 3. The world's deepest oceanic area. Depths greater than 4,000 fathoms are shown in solid black. *Challenger's* deepest sounding is represented by the white spot in the Mariana Trench

by *Challenger* in the Mariana Trench surpasses that reported by *Galathea* by 177 fathoms (323 metres) and exceeds the Cape Johnson Depth by 200 fathoms (366 metres).

As regards the wire sounding, the practice of Wüst in assessing true depth from the study of the differences between wire and echo depths at different wire-angles is set out in his admirable *Meteor* report[1]. He considers that the most likely value for the true depth is the mean of the best-corrected wire and echo depths.

Both echo depths obtained during the first visit by the low-frequency method were appreciably less than the wire depth. This is surprising, because due to spurious echoes being produced by reflexion from the surrounding walls, low-frequency echo soundings in deep-sea trenches usually give values which are greater than the true depths. These echo depths, therefore, which average 5,650 fathoms (10,333 metres) can be accepted with special confidence.

If Wüst's procedure were followed, the acceptable value for the sounding first reported by *Challenger* would be the mean of 5,872 and 5,650 fathoms = 5,761 fathoms (10,536 metres). This is greater than the Cape Johnson Depth by 21 fathoms (39 metres) and is almost identical with that obtained by *Galathea*.

These new *Challenger* soundings show that it is no longer the Philippines Trench which holds pride of place as the deepest oceanic area in the world, but the smaller Mariana Trench (Fig. 3). This region was first visited some seventy-five years ago by the illustrious namesake of the present *Challenger* during her famous world cruise. About fifty miles to the east of the scene of the new deep sounding, the earlier *Challenger* found what was the greatest depth of her cruise, namely, 4,475 fathoms (8,184 metres); in 1912 Sir John Murray named it the Challenger Deep[5].

The U.S.S. *Nero* also found deep soundings in the vicinity in 1899, and it was upon these, together with the original *Challenger* sounding, that Otto Krümmel based his chartlet of the Mariana Trench drawn in 1907[6]. Between 1925 and 1931 the area was extensively sounded by the Japanese naval vessels *Mansyu*, *Kosyu*, *Yodo* and others[7], and a maximum sounding of 5,366 fathoms (9,814 metres) was obtained about ten miles away from the position of the new *Challenger* depth.

It is a matter of high interest to have revealed that the greatest ocean depths exist in the Mariana Trench, and it is a particular pleasure to record that the discovery was made by a British naval vessel bearing the honoured name of *Challenger*.

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[1] Wüst, G., "Meteorwerk", **4**, 1.
[2] Pinke, "Snellius" Reports, **2**, Part 2, Chapter 1, Table 1, pp. 53–55 (Utrecht, 1935).
[3] Wüst, G., *Die Erde*, **3–4**, 213 (1950–51).
[4] Hess, H. H., and Buell, jun., M. W., *Trans. Amer. Geophys. Union*, **31** (June 1950).
[5] Murray, Sir John, and Hjort, J., "The Depths of the Oceans", Map. 11, p. 129 (1912).
[6] Krümmel, O., "Handbuch der Ozeanographie", **1**, 124 (1907).
[7] Sigematu, R., Record of Oceanographical Works in Japan, **5**, No. 2 (Tokyo, June 1933).

PROTECTIVE COATINGS OF METALS UNDER CONDITIONS OF ATMOSPHERIC CORROSION

A JOINT meeting of the Corrosion Group of the Society of Chemical Industry with Commission No. 4—Protection contre la Corrosion—of the Association Belge pour l'Étude, l'Essai et l'Emploi des Matériaux took place on March 14 at the Iron and Steel Institute, London, with the chairman of the Corrosion Group, Dr. W. H. J. Vernon, presiding. The Belgian Commission was represented by Prof. P. Ercuisse, president of the Association, Dr. M. Van Rysselberge, secretary, and by MM. Edouard Oostens, Emile Oostens, J. Lagae, L. Frankson and D. Bermane, the Commission's investigator.

After Dr. Vernon had opened the morning session by welcoming the Belgian delegates, Prof. Ercuisse read a paper summarizing the work of the Belgian Commission on protective coatings, which was formed in 1937. Research undertaken so far has been mainly

on the formulation of paints and their performance in natural weathering tests. The first series of tests was begun in 1937 at eleven stations representing a wide range of atmospheric conditions. Some of the observations had to be abandoned because of the German occupation, during which the Commission's first investigator, M. Jeunehomme, unfortunately lost his life; but results covering ten years of exposure are now available for most of the stations. Red lead in linseed oil has proved to be the best of a number of priming paints for steel. This observation was confirmed by the results of more recent tests, in which only two other paints gave results comparable with those for red lead. These were a paint of zinc and zinc oxide in linseed oil and an aluminium paint in a coal tar pitch medium. Zinc chromate paints in alkyd media and chlorinated rubber paints gave only moderate results. It was also found that the durability of painting schemes was longer by up to 30 per cent on low-alloy steels containing copper, or chromium and aluminium, than on ordinary steel. Accelerated weathering tests are made in parallel with the field-tests, using a test cycle that has been evolved from the original Gardner wheel. The breakdown of the paint film is followed by determining its hardness, adhesion, elasticity and permeability. The most recent test programme now being initiated is concerned with sixty new priming paints for steel, in which some of the newer paint-forming materials have been incorporated. The media include certain alkyds and styrenated oils; red lead, basic lead sulphate, lead cyanamide, barium chromate, zinc chromate and zinc dust are being used as pigments.

Dr. S. G. Clarke and W. N. Bradshaw, of the Armament Research Establishment, Ministry of Supply, reported some tests on the protective value of metallic coatings under sheltered conditions in marine atmospheres. Specimens of steel sheet carrying metallic coatings of various metals and thickness were exposed inside a ventilated box at a site on the north coast of Cornwall. After three years of exposure, no rusting had occurred on specimens with coatings of zinc or cadmium 0·3 mil thick; but specimens with an equally thick coating of tin, lead or lead–tin alloy had rusted severely; a similar coating of tin–zinc alloy had permitted moderate rusting. Specimens with a sprayed aluminium coating 3 mils thick were unrusted. The main conclusion to be drawn from the results is that, despite the present shortage of the metals concerned, a reasonable thickness of coating must be maintained if protection is to be ensured. An interesting observation was that all the corrosion products contained more sulphate than chloride, whereas the reverse holds true for sea salt.

F. Fancutt and Dr. J. C. Hudson described the researches of the Protective Coatings (Corrosion) Sub-Committee of the British Iron and Steel Research Association. Work on paints has shown the importance of correct surface preparation of steel for painting. The life of a given painting scheme was found to be four or five times greater on a properly prepared surface obtained by pickling or sand-blasting than on the surface resulting from the common practice of weathering and hand-cleaning. Research on the formulation of paints for protecting steel against atmospheric corrosion is proceeding under the direction of a joint technical panel of the Sub-Committee with the paint industry. Several series of tests on priming paints are in progress. So far, the best results have been obtained from paints bound with linseed oil, partly because these paints naturally tend to give thicker films than paints in synthetic media. A paint pigmented with forty parts by weight of red lead, forty parts of white lead and twenty parts of asbestine and bound with linseed oil has proved equal in protective efficiency to a red lead in linseed oil priming-paint and has better can stability and better brushability. Recently a systematic study of the formulation of protective paints based on tars, pitches and bitumens has been undertaken.

Tests on metallic coatings have established that the protective value of a zinc coating is mainly determined by its thickness and that the process of application, whether hot-dipping, cementation, electrodeposition or spraying, has little influence. A coating thickness of roughly 3 mils (say, 1·7 oz. of zinc per sq. ft. of surface) is needed to protect steel for five years in an industrial atmosphere like that of Sheffield. Approximately the same thickness of sprayed aluminium (0·8 oz. per sq. ft.) will protect steel equally well.

Dr. P. T. Gilbert contributed a paper on the effect of impurities in the metal on the rate of corrosion of zinc and galvanized coatings in the atmosphere. Exposure tests made by the British Non-Ferrous Metals Research Association have shown that the usual impurities have little effect on the resistance of zinc to atmospheric corrosion. For example, the test duration to failure for a series of coatings containing appreciable percentages of aluminium, antimony, cadmium or tin, was essentially proportional to the coating weight and independent of the composition of the galvanizing bath; the addition of 2 per cent of copper did, however, reduce the corrosion of sheet zinc by about 20 per cent. The corrosion-rate of zinc increased with time, and at the end of fourteen years of exposure was roughly twice the rate at the beginning. This increase is due to the roughening of the surface.

The afternoon session began with a paper by D. Bermane on difficulties encountered in open-air exposure testing. These fall into two categories: those associated with the organization of the tests, and those associated with the interpretation of the results. The former include the formulation of the paints, the choice of the exposure stations, and the number and shape of the specimens. The value of tests on painting schemes is greatly enhanced when full details of the paint formulations are known. There is no need to maintain a large number of stations, because the order of merit of different painting schemes does not vary greatly from one station to another. It would, however, be unwise to restrict tests to a single station, and the Belgian Commission has compromised by reducing its number of stations from eleven to four. Marked differences are sometimes observed between the behaviours of duplicate specimens, so that it is desirable to conduct each test on four or five replicates. This course may lead to an unwieldy number of specimens; some specimens have therefore been used for tests on more than one painting scheme. The shape of specimen is important. Hitherto the Belgian Commission has used a bent specimen consisting of vertical, inclined and horizontal surfaces, and has made observations on both the outer and the sheltered faces. Unfortunately, the ratios of the lives of the paints on the six surfaces are not constant for different painting schemes, so that a specimen of simpler shape might be preferable.

Difficulties in the interpretation of results have arisen from the use of the German scale *DIN* 4710 for assessing breakdown, mainly because the grades of this scale are too widely separated. Moreover, it is based solely on estimates of the rusting of the basis steel and takes no account of other important factors, such as chalking and cracking of the paint film.

In a second paper, M. Bermane compared the results of accelerated tests on paints by means of the Belgian Commission's cycle with those of a French cycle (Gaz de France), the weatherometer and the A.S.T.M. (American Society for Testing Materials) continuous salt-spray test, respectively. Of the four methods, only the French test and the Belgian test gave good correlation with outdoor tests.

R. St. J. Preston read a paper on the mechanism of the processes involved in atmospheric corrosion testing. Experiments at the Chemical Research Laboratory, Teddington, have shown that no direct reaction of any magnitude takes place between water vapour and iron at ordinary temperatures and that condensation of water must occur on the iron surface if appreciable corrosion is to result. This was demonstrated by an ingenious colour film showing tests on two similar steel specimens suspended in beakers containing a warm solution of sulphur dioxide. The bottom of one specimen dipped into the solution, so that the transfer of heat prevented condensation on it. This specimen remained unrusted. The other specimen, which was hung clear of the solution, experienced condensation and rusted rapidly.

General discussions were held at the end of both the morning and afternoon sessions, in which T. H. Turner, J. F. Kayser, Dr. J. E. O. Mayne, C. E. Richards, Dr. E. E. Longhurst, the authors of papers and others took part.

In the evening the Council of the Society of Chemical Industry entertained the Belgian visitors to dinner at the May Fair Hotel. After the loyal toast, Dr. W. H. J. Vernon proposed the health of the guests. In a simple and moving reply, Prof. Erculisse referred to the strong ties of gratitude and affection uniting Belgium and Great Britain. Here the dictum of Pascal—"Le cœur a des raisons que la raison ne comprend pas"—did not apply.

JOHN LELAND, *c.* 1506–52

By JOAN M. EYLES

JOHN LELAND, famous as an antiquary and topographer, died on April 18, 1552. For the previous five years his many manuscripts had lain untouched, since the melancholy day when Leland "by a most pitifull occasion fell about his wits". In fact, his detailed notes about his journeys through the length and breadth of England and Wales remained unpublished for a hundred and fifty years after his death, though they were copied and used by many later antiquaries. Then during 1710–12, the "Itineraries" were edited and published by Thomas Hearne, the assiduous librarian of the Bodleian, in which many of Leland's manuscripts had found a worthy home. Nearly two centuries later, between 1907 and 1910, the "Itineraries" were re-published with maps and many annotations by Miss L. Toulmin Smith.

The "Itineraries" are a mine of information for the historian and antiquary, and most local historians quote Leland as their earliest and most interesting source of information, not only for descriptions of monuments and ancient buildings, but also for topographical details of rivers and streams, woods and cultivated ground. Less well known are his many observations concerning the mineral wealth of the country. Though few in number compared to his antiquarian notes, they provide valuable information for the economic historian and also have some interest for the geologist. Moreover, though Leland was no naturalist, he sometimes recorded the "commune fische" of different pools and lakes.

The exact date and place of Leland's birth is unknown, but he attended St. Paul's School, and graduated B.A. from Christ's College, Cambridge, in 1522. Later he studied at Oxford, and after two or three years there went to Paris, apparently with some sort of foreign scholarship. He returned to England about 1529, and in 1533 received a royal warrant from Henry VIII which permitted him "diligently to serche al the libraries and monasteries of this youre noble reaulme". While travelling about the country engaged on this task, Leland made the numerous notes which supply such a detailed description of England during the decade 1535–45. Interested in the local industries, he noted the quarries for stone and the workings for various minerals. He gave a long account of the salt works at Droitwich; and in his description of Cornwall frequently referred to the many "tynne werkes", remarking how some of the streams were "clene chokid with the sandes" washed down from these works. He also noted such coal pits as he saw, including "pittes of se coles" at Wakefield and Gateshead, and others in Staffordshire, South Wales and Lancashire. His observations about the occurrence and working of coal are of considerable interest. Thus he noted: "The vaynes of the se coles ly sometyme open upon clives [cliffs] of the se, as round about Coket Island and other shores, and they be properly caullid se coale; but they be not so good as the coles that are diggyd in the inner parte of the lande"; and he added, "The craft is to cum to it with leste paine in depe digging", an observation which would no doubt be endorsed by the modern open-cast miner. Sometimes he noted the nature of the soil, whether chalky, gravelly or, as in "Venteland" (Monmouthshire), "of a darke reddische yerth ful of slaty stones". In a few places he noted fossils, such as the "myghty shells of great oysters turned in to stones" near Alderley in Gloucestershire, and "stones figurid like serpentes wounde into circles" (that is, ammonites) near Keynsham, Somerset. These may well be the first references to English fossils.

In an address that Leland presented to his royal master as a "Newe Yeares Gyfte" in 1546, he stated his intention of preparing, "if God sende me life to accomplish my beginninges", a map of the "worlde and impery of England", which would be engraved on a "quadrate table of silver". He also intended "such a description to make of youre reaulme yn writing, that it shaul be no mastery after, for the graver or painter to make a like by perfect exemple". By a strange coincidence, the first important engraved map of the British Isles was published at Rome in 1546. Its authorship has been attributed to the exiled George Lily, a prominent Catholic and son of William Lily, grammarian and Leland's former

headmaster at St. Paul's. Because its accuracy is remarkable for the time, it was recently suggested[1] that it was actually prepared in England by "a group of English patriots, inspired by the antiquarian and historical ardour of the Renaissance". If this was the case, one wonders whether the travelled and erudite Leland was consulted during its preparation; although, as he was an ardent supporter of the king, it is unlikely he would have approved of the publication in Rome of a map such as he himself had intended for the greater glory of Henry VIII.

Leland was buried in St. Michael-le-Querne, near St. Paul's, where, according to Stow, in his "Survey of London", there existed a monument in memory of him; but the church was destroyed in the Great Fire of 1666.

[1] Lynam, E., *Geog. J.*, **116**, 8 (1950).

OBITUARIES

Prof. F. T. Brooks, C.B.E., F.R.S.

By the sudden death of Prof. F. T. Brooks on March 11, Cambridge has lost a man who played an important part in the development of the study of botany, and Britain is deprived of the wisdom and service of a leader in the field of mycology and plant pathology. Born at Wells in Somerset in 1882, he left Sexey's School, Bruton, in 1898, and entered Emmanuel College, Cambridge, in 1902. Like so many other undergraduates of that time, he fell under the spell of Marshall Ward's brilliant teaching of botany, and he gained first classes in both parts of the Natural Sciences Tripos, afterwards to commence research on plant diseases under Ward's direction. Prof. Marshall Ward died in 1906, and Brooks was left to carry on the specialized teaching of plant pathology in the Botany School. In 1914 he spent a year in the Federated Malay States investigating the diseases of rubber, and on his return he joined the team of investigators constituting the original Food Investigation Department of the Government. At the end of the War, which had seriously affected his personal happiness, he became University lecturer and continued his researches on diseases of crop plants. The peculiar silver-leaf disease, then very prevalent in Victoria plums, occupied much of his attention, and he again attacked some of the problems concerning the rust fungi of cereals, an important field in which Marshall Ward had done pioneer work.

During the years that followed, Brooks was responsible for the development of the study of mycology and plant pathology at Cambridge. In this he was highly successful; he trained a large number of men and women for research positions in Britain, the Dominions and the Colonies. His eminence led to his election as president of the British Mycological Society in 1922 and he played a considerable part in extending the scope of that Society, later recognized by his appointment as one of its honorary members. His work at Cambridge received considerable impetus and assistance by the gift to the University from the Rockefeller Trustees of a large sum of money for the building and equipment of a modern department of plant pathology, together with provision for a field-station for experimental work. Brooks became reader in botany, and was elected to a fellowship in his College where he played an active part.

On the resignation of Sir Albert Seward in 1936 Brooks was elected professor of botany, and he occupied this position with distinction until he reached the age of retirement in 1948. During this time he devoted much attention to the development of the Cambridge Botanic Garden, and to the plans for its improvement by the use of the Reginald Cory bequest. He took an active part in University administration and served as president of the Cambridge Philosophical Society during 1945–47.

His activities were not confined to Cambridge. After holding the office of president of Section K (Botany) of the British Association in 1935, he was biological secretary of the Association until 1946. He became a Fellow of the Royal Society in 1930, and served on its Council during 1939–44. He was secretary of the First Imperial Botanical Conference in 1924, and one of the two secretaries of the Fifth International Botanical Congress in 1930. Later he gave much time and energy to the work of the Agricultural Research Council, of which he was a member from 1941 to the time of his death, apart from an intermission of three years; of late this work taxed his physical strength very severely. In 1947 he received the well-merited award of the C.B.E., he was made an honorary LL.D. of St. Andrews, an honorary Fellow of the Royal Society of Edinburgh and of the Botanical Society of Edinburgh.

Brooks was the author of a large number of original papers on mycology and plant pathology, he wrote an important book on "Plant Diseases" and he edited and largely re-wrote a new edition of D. H. Scott's elementary text-book of botany. Last year he was responsible for the publication of an interesting little book on the auriculas, written by his friend Sir Rowland Biffen, which the author had left in manuscript at the time of his death.

A remarkably modest and friendly man, with wide interests in plants of all kinds, in horticulture and in agriculture, an excellent teacher who took a great interest in his students, he had a host of friends all over the world who will remember him with gratitude and affection.

H. Hamshaw Thomas

F. T. Brooks was a man of marked and unusual personality, some features of which seemed to hark back to an earlier and more robust age than ours. In him the child was certainly father of the man, for as a pupil at Sexey's School, Bruton, the leading traits of character that distinguished him in later life were clearly discernible. Although he was not an outstanding figure at games or athletics, he entered with great zest and *joie de vivre* into every activity of school life. As a scholar he was marked by his all-round ability, except in one respect: he did not shine in the workshops devoted to woodwork and metalwork. A thorough West-countryman by birth and upbringing, he took an intense interest in country life, and his inherent love of Nature quickened and grew under the fostering influence of his headmaster, W. A. Knight, a great pioneer of nature-study and of science-teaching in schools. Frequent botany rambles, in a countryside renowned for the wealth and variety of its flora, formed an important part of the school curriculum, and to the end of his life this apt pupil of an inspiring master remained an ardent field-botanist.

The path leading eventually to the chair of botany at Cambridge and a fellowship of Emmanuel College

was beset with formidable obstacles; but Brooks was endowed with great resolution and persistence. At Sexey's School he learnt more than the formal subjects of teaching: he acquired the habit of methodical working, even at unattractive tasks, and also of concentration. In later life, as at school, his forceful and forthright character was tempered by friendliness and geniality, and mellowed by a robust sense of humour. He possessed unbounded energy, both physical and mental. At Cambridge he played tennis and squash rackets with great zest, and until fairly late in life he took an early morning dip in the river as a regular practice throughout the year. Often, also, in going home to his beloved West Country he would take train to Bristol and walk the twenty miles over the Mendips to Wells. He was fond of travel, and his botanical wanderings took him as far away as Malaya and Australia.

Besides his heavy teaching and administrative duties at the Botany School, he took a leading part in the councils of Emmanuel College. He was also a governor of his old School, a member of an unusual number of committees and commissions, and he served for a period as joint general secretary of the British Association. As a chairman he was particularly effective; for his firmness and honesty of purpose, his shrewd common sense, and his outspoken and good-tempered judgments appealed to all. His mind was of a strongly practical cast, and so it came about that his interests ranged far beyond the limits of academic mycology and botany into their applications in gardening, fruit-growing, cider-making, agriculture, rubber-cultivation, and the like.

The short and sturdy figure with the purposeful air, the ruddy good-humoured countenance, the strong West-country voice, the cheery greeting, the hearty and infectious laugh, the warm welcome, the unnumbered acts of kindness, will not lightly be forgotten:

"But now we hope his kindly feäce
Is gone to vind a better pleäce;
But still, wi' vo'k a-left behind
He'll always be a-kept in mind".

JOHN READ

Lord Lindsay of Birker, C.B.E.

LORD LINDSAY OF BIRKER, Master of Balliol College in the University of Oxford from 1924 until 1949, and latterly principal of the University College of North Staffordshire, died on March 18 at the age of seventy-two. His writing and teaching were in the field of philosophy; but his influence in education extended far beyond his subject and outside the College where most of his working life was spent.

Only a few of his many activities can be mentioned here. He played a large part in the establishment and development in Oxford of the Honour School of Philosophy, Politics and Economics, believing that the study of philosophy should not always depend on a classical background, but should be related to contemporary social problems. He also urged, though in vain, the foundation of a school based upon the joint study of philosophy and the principles of natural science. He was vice-chancellor of the University of Oxford during 1935–37, a period which saw many important developments, including the Nuffield bequests for medicine and social sciences. During the Second World War he was chairman of the Oxford Joint Recruiting Board, and handled with humanity and understanding the many problems arising from the call-up of students from the University. When an international commission was appointed after the War to consider the future of the German universities, Lindsay was the only British member, and his wide knowledge and good humour eased many difficult situations. All these activities (and many others) were not just the automatic accretions of an able administrator, but were causes for which he felt deeply and worked unsparingly.

Lord Lindsay was always keenly interested in education outside the conventional framework of academic study, for example, in the Workers' Educational Association and education in the Armed Forces. It was typical of his pioneering spirit that at the age of seventy he accepted the invitation to become the first principal of the University College of North Staffordshire, in the foundation of which he played a leading part. Here he was able to initiate an educational experiment which embodied several of his own ideas, notably a first-year course common to students of all faculties.

Many institutions owe much to Lord Lindsay, and many individuals will remember him with affection as an inspiring teacher and colleague.

Mr. John Wylie

JOHN WYLIE died at Larne, Co. Antrim, on February 29 at the age of eighty-six, after a short illness. He rendered great service to the Queen's University of Belfast, of which he was a B.A. (1890) and an M.Sc. *honoris causa* (1948), during the forty-eight years that he was a member of the staff of the Physics Department. As demonstrator (1897–1919) and lecturer (1919–30) he was given charge of the undergraduate teaching laboratories by the late Prof. W. B. Morton. These were at first in the old college block but later, after 1914, were in the present physics building, which was erected to Wylie's plans. When he reached the normal retiring age in 1930, the post of workshop superintendent was created for him. Especially in the period between 1920 and the time of his ultimate retirement, when he had the able assistance of Mr. Adam Buick as mechanic, he produced much beautiful apparatus for teaching and research throughout the University, with the most meagre shop equipment. He also acted for many years as secretary to both the Matriculation Board and the General Board of Studies of the University. Wylie was a modest man of simple tastes, hiding great kindness behind a superficially abrupt manner. In younger days, he was a keen yachtsman, and well known as the designer of the Waverley yacht. He remained active, mentally and physically, until the last month of his life. He is survived by his wife.

K. G. EMELÉUS

WE regret to announce the following deaths:

Prof. Paul Dienes, formerly professor of mathematics in the University of London (Birkbeck College), on March 23.

Dr. L. Dobbin, formerly reader in chemistry in the University of Edinburgh, on March 3, aged ninety-three.

Mr. Henry Woods, F.R.S., formerly University lecturer in palæozoology, Cambridge, on April 4, aged eighty-three.

NEWS and VIEWS

Chairmanship of the Advisory Council on Scientific Policy : Sir John Cockcroft, C.B.E., F.R.S.

In *Nature* of March 8 (p. 395) it was announced that Sir Henry Tizard was retiring from the chairmanship of the Advisory Council on Scientific Policy. He is now retiring also from the chairmanship of the Defence Research Policy Committee, and will be succeeded by Sir John Cockcroft. Sir John's scientific career, which brought him the award of the Nobel Prize for Physics in 1951, is well known, and his extraordinary ability as an administrator and organizer of scientific research is scarcely less widely appreciated. The Atomic Energy Research Establishment, of which he was appointed director in 1946 (see *Nature*, **157**, 128 ; 1946), has played an essential part in the large-scale development of the British atomic energy programme, and has grown under his direction into one of the foremost centres of nuclear research in the country. The successful construction of two experimental piles and of four major nuclear instruments (of which Sir John gave an admirable account recently before the Royal Society) is a tribute to his ability to inspire a large and diverse body of scientific and technical effort. It is a source of continued admiration to those who know him that he is able to combine the direction of a large establishment with a detailed and up-to-date knowledge of scientific developments both at home and overseas. There are many projects, not only within the Government service, but also in universities and in industry, which owe much to his advice and encouragement, given most generously and often in person. It is gratifying to know that his new post will not take him from Harwell ; the Government is fortunate in being able to unite two vital interests under a single head who brings to his responsibilities such a wealth of wisdom and experience.

Physics in Western Australia : Prof. A. D. Ross, C.B.E.

Prof. A. D. Ross retires in May from the chair of physics in the University of Western Australia after an association with the University lasting nearly forty years. Born in Glasgow in 1883, he had a distinguished career at the University of Glasgow and at Göttingen, where his interests were, particularly, in the fields of magnetism and spectroscopy. Upon the foundation of the University of Western Australia, Prof. Ross was appointed in 1912 to the chair of mathematics and physics, arriving in Perth to take up duties early in 1913, and in the next year classes commenced. When mathematics and physics were separated in 1929, he took over the chair of physics. Except for a visit to Europe during the First World War and again in 1951, Prof. Ross has given uninterrupted service as a teacher, scientific investigator and administrator, and he is the last in office of the eight foundation professors. Prof. Ross has played a distinguished part in building up the University tradition in the State of Western Australia both as professor of mathematics and of physics and in the numerous posts he has held in scientific and other organizations inside and outside the University. He and other professors brought from Britain in the early days of Australian universities had the distinction and privilege of being agents for the transmission of high educational standards and sound cultural traditions from the European universities to the young country of Australia. Prof. Ross has been chairman of the Music Advisory Board, president of the Australian Branch of the Institute of Physics, president of the Royal Society of Western Australia, chairman of the Western Australian Division of the National Research Council, and he has served science and the community in many other ways. During the Second World War Prof. Ross was appointed a member of the Commonwealth Optical Munitions Panel, deputy director of camouflage and consulting physicist to the Royal Australian Navy. He was made a C.B.E. in 1949.

Prof. C. J. B. Clews

Dr. C. J. B. Clews, who succeeds Prof. A. D. Ross, has chosen as his main field of inquiry the elucidation of organic crystal structures by means of X-ray diffraction analysis. His most recent work of this type was done in the Cavendish Laboratory between 1945 and 1948, where he held an I.C.I. fellowship. Since 1948, he has been a principal scientific officer at the National Physical Laboratory, in charge of X-ray and electron diffraction investigations and electron microscopy, with particular reference to metals and alloys. Prior to the War, before taking up a post as lecturer in physics at Queen Mary College, University of London, he worked with Dr. (now Prof.) Kathleen Lonsdale at the Royal Institution, and spent a year in Prof. H. Mark's laboratory in Vienna. Between 1940 and 1945, Dr. Clews was active in the scientific war effort, first in the Ministry of Supply extra-mural project under Sir Lawrence Bragg on the location of long-range artillery, then in the Air Defence Research and Development Establishment. During 1942–44 he was a member of the Army Operation Research Group and was concerned, among other problems, with the location of V2 launching sites. After the landings in France, he served in Western Europe with the rank of major on the Scientific Intelligence Staff. Dr. Clews, who was born in 1912, received his early training in physics under Prof. H. R. Robinson at Queen Mary College, London, and received his Ph.D. for research there on the electrical conductivity of strong electrolytes. His intention is to develop work along two main lines in Western Australia ; on one hand, he proposes to continue X-ray crystallographic researches on organic molecules, and, on the other, to develop the use of soft X-ray spectroscopy in studies of the solid state.

Gold Medal of the Professional Institute of the Public Service of Canada : Mr. A. Thomson, O.B.E.

The Gold Medal for 1952 of the Professional Institute of the Public Service of Canada has been awarded to Mr. Andrew Thomson, controller of meteorological services of the Meteorological Division, Canadian Federal Department of Transport, for his outstanding contributions to national and world well-being. Born in Dobbinton, Ontario, in 1893, Mr. Thomson graduated in physics in the University of Toronto, and in 1917 was appointed Townsend student in physics in Harvard University. He later joined the Department of Terrestrial Magnetism of the Carnegie Institution of Washington, and in 1919 went on a solar eclipse expedition to the interior of Brazil. He was in charge of the investigations on atmospheric electricity on a 26-month cruise around the world aboard the research ship *Carnegie*. In 1922 Mr. Thomson went to the Apia Observatory, Samoa, first as scientific adviser and in the following year as director. In 1929 he was appointed aerologist

for the Dominion of New Zealand. Returning to Canada two years later, Mr. Thomson joined the Meteorological Division, being first chief of the Research Division, followed by promotion to assistant controller of meteorological services in 1939 and controller in 1946. During his term in the post of controller, the meteorological service in Canada has been expanded considerably, and, in particular, weather stations and an aerial service have been set up in the arctic regions, a development which is of great importance both from a defence point of view and for the pursuance of new knowledge.

Melchett Medal of the Institute of Fuel

THE Institute of Fuel has awarded the Melchett Medal for 1952 to Dr. D. T. A. Townend in recognition of his outstanding contributions to the science of combustion, particularly in the field of higher hydrocarbons. Dr. Townend is at present director general of the British Coal Utilization Research Association; he was formerly Livesey professor of coal gas and fuel industries in the University of Leeds, and was president of the Institute of Fuel during 1948–50.

Technological Education in Great Britain

REPLYING in the debate on education in the House of Commons on March 25, the Parliamentary Secretary to the Ministry of Education, Mr. Kenneth Pickthorn, said that no very positive declaration could be made at present about technological education. The Government, he said, is fully aware of its importance, and he emphasized that the best technological education in Britain is in no way inferior to that anywhere else; he added that the real difficulty is that we were not producing enough of the best type of technologists. The matter is one which, he suggested, is primarily the concern of the universities and not the Minister of Education. Mr. Pickthorn's comments demonstrated how unsatisfactory the present situation is; but he gave no clear indication whether or not the Government proposes to place on the shoulders of the University Grants Committee the responsibility for general oversight of technological education. Nor will misgivings be removed by his further admission that the Minister of Education is endeavouring to encourage the use of technical colleges for the production of technologists and intends to do all she can to assist in that way.

Air Pollution in the United States

THE second National Air Pollution Symposium, sponsored by the Stanford Research Institute in co-operation with the California Institute of Technology, the University of California, Los Angeles, and the University of Southern California, will be held during May 5–6 at the Huntington Hotel, Pasadena, and will follow the general pattern of the first symposium, held in 1949. The meeting will be attended by men of science, engineers, technical workers and executives interested in the various aspects of air pollution as it affects industrial communities. The programme will cover general topics of new techniques in sampling, analysis and instrumentation, fundamental chemistry and physics of the atmosphere, and the contribution of internal-combustion engines and biological aspects of atmospheric contamination, as well as the views of management on the problem. Further information can be obtained from the headquarters of the Symposium at Room 332, 612 South Flower Street, Los Angeles 14, California.

Charts of the History of Science

THE Science Museum (South Kensington, London, S.W.7) has recently published a set of three charts, bearing the title "Synopsis of Events connected with Science" (29 × 26¾ in.; from the Museum; 1950; 5*s*. 3*d*. a set), in which the period of time covered extends from the "age of cosmos", indicated as "Infinity/*c*. 30,000 million B.C.", to the middle of the present century. After a short introductory section on the approximate stages leading to primitive man, *c*. 1,000,000 B.C., the Synopsis is divided into six columns labelled history, applied science, physical science, biological science, basic ideologies and art. The lettering used is such that these charts can conveniently be read when affixed to a wall. With such type, the size of the charts admits a very large number of entries in each column; proportionately more space is allotted to recent years, so that the third chart covers the period 1850–1950, with a blank space for future entries. As befits a publication of the Science Museum, special attention has been paid to detail and clarity in the preparation of these charts. Like all time-charts, however, they are open to criticism—either of omission or commission—according to the particular purpose in view; but the student and general reader will find in the appropriate columns a useful survey of the history of science.

Earthquakes in South Africa during 1938–44

IN a recent publication entitled "Earthquakes in South Africa" (Department of Mines, Geological Series Bulletin No. 20. Pp. 14; Pretoria: Govt. Printer, 1951; 1*s*. net), L. J. Krige and B. D. Maree have given accounts of eleven local earthquakes which occurred between 1938 and 1944. The data were chiefly obtained from replies to seventy-three questions on a questionnaire circulated to magistrates, postmasters and postal agents. The answers enabled the earthquake intensity at any place to be assessed on the Modified Mercalli Intensity Scale of 1931. Instrumental data were obtained from readings from seismograms obtained at the Cape Town and Johannesburg Observatories. Details of earthquakes are given in the paper under the following headings: intensity and epicentre; time (in South African Standard Time) and duration; foreshocks and aftershocks; direction of propagation; damage; possible geological explanation; instrumental data; and analysis of seismograms. The earthquakes occurred in Zululand (epicentral intensity 5) just after 10 p.m., February 10, 1938; Bethlehem (5) just after 1.40 a.m., March 1, 1940; Mozambique (6), 8.18 p.m., May 19, 1940; Aberdeen (5), 3.45 p.m., October 13, 1940; Tzaneen (6), 11 p.m., November 10, 1940; Namaqualand (5), 8.30 p.m., October 23, 1941; Port Shepstone (6), 6.50 a.m., November 1, 1942, together with six subsidiary shocks; Prieska (5), 8.45 p.m., November 24, 1943; Beaufort West (3), during the night of August 28, 1944; Vryheid (5), 10.15 a.m., September 17, 1944; Ladybrand (5), 10.55 p.m., November 12, 1944. The greatest intensity (6), mentioned above, indicates such an intensity as would awake people from sleep, set church bells ringing and damage poorly constructed buildings. In some cases of very local earthquakes, the initial movement (at the focus) was near enough to the earth's surface for the authors to be able to correlate it with geological phenomena such as surface fault movement; but in greater earthquakes the focus

was much deeper and, as stated in the paper, probably related to movements affecting the entire sub-continent. The authors regret that there are not more observatories in South Africa equipped to record earthquakes instrumentally. A map showing isoseismal lines for the earthquakes described is given in the paper.

Colonial Service: Recent Appointments

The following appointments have recently been made in the Colonial Service: C. A. S. Hynam (agricultural superintendent, Antigua, Leeward Islands), agricultural officer, Trinidad; W. V. Calder (assistant conservator of forests, Federation of Malaya), assistant conservator of forests, Tanganyika; D. H. Hodgson (chief conservator of forests, Sierra Leone), development secretary, Sierra Leone; J. R. Lockie, J. C. Mallam and N. S. Stevenson (assistant chief conservators of forests, Nigeria), chief conservators of forests, Nigeria; J. H. Harris (senior metallurgist, Tanganyika), assistant director of geological survey, Tanganyika; A. M. Quennell (senior geologist, Tanganyika), assistant director of geological survey, Tanganyika; R. Walls (senior geologist, Nigeria), assistant director of geological survey, Nigeria; G. M. Gibson (director, Lands and Mines Department, Tanganyika), director, Lands and Surveys Department, Tanganyika; P. C. Bartrum (meteorologist, Sierra Leone), senior meteorologist, Nigeria; H. Fell (national income tax statistician, East Africa High Commission), deputy registrar of statistics, Federation of Malaya; H. O. Walker (senior meteorologist, Nigeria), chief meteorologist, Gold Coast; B. Baldwin, plant pathologist (agriculture), Uganda; B. Butters, agricultural chemist, Kenya; M. H. F. Cooper, agricultural officer, Tanganyika; T. J. Crowe, entomologist, Kenya; W. Hoole and A. K. Murray, agricultural officers, Nigeria; J. S. Robertson, plant pathologist, British Guiana; J. C. Templer, agricultural officer, Kenya; Miss E. V. Walker, specialist plant breeder, Gold Coast; T. B. Wilson, agricultural economist, Federation of Malaya; J. C. D. Watts, scientific officer (marine biologist), West African Fisheries Research Institute, Sierra Leone; A. M. Mackie, veterinary officer, Uganda; M. S. Tubridy, veterinary officer, Tanganyika; C. P. Haddon-Cave, statistician, East Africa High Commission; P. H. Creutzberg, marine biologist, West African Fisheries Research Institute, Sierra Leone; H. S. Haynes, fisheries officer, Commerce and Industries, Nigeria; E. N. Jennings, agricultural superintendent, Nigeria; A. E. Olazyna-Marzys, chemist, Uganda Mineral Resources (Research) Committee; B. E. Maxwell, scientific officer, Central Africa Freshwater Fisheries Research, Northern Rhodesia; I. T. Twyford, research officer, soil survey, Fiji; V. J. Wooller, meteorological assistant, Bermuda.

The Original Orrery

The original orrery, a splendid and ornate instrument, has been lent by the present Earl of Cork and Orrery to the Science Museum, London. It is temporarily displayed among the Astronomy Collections; but will be transferred to the Special Exhibition, "The British Clockmaker's Heritage", when that opens in May. Orreries are designed to show the motions of the sun, moon and earth, and to ensure accurate representations of these motions they contain complicated and delicate wheel-work. It is therefore not surprising that their construction was undertaken by clockmakers such as Graham and Tompion. The present example was made by John Rowley in about 1716 for Charles Boyle, fourth Earl of Orrery, in whose honour these mechanisms now bear their present name of 'orrery'.

Announcements

Prof. Maurice Lemoigne, head of the Department of Fermentation of the Pasteur Institute, Paris, has been elected a member of the Section of Rural Economy of the Paris Academy of Sciences in succession to the late Prof. Emile Schribaux.

Mr. Arnold Frobisher, secretary of the Wool Industries Research Association since its formation in 1918, is resigning, on account of ill-health.

The Aluminium Development Association is offering a research scholarship of £400 a year for two years to encourage and facilitate research in the application of light alloys to ship construction. Full particulars of entry, which closes on June 30, can be obtained from the Secretary, Institution of Naval Architects, 10 Upper Belgrave Street, London, S.W.1.

The Colorado School of Mines at Golden, Colorado, is offering a scholarship to a male student from England, Wales or Northern Ireland for the academic year 1952–53. The scholarship covers tuition fees only, amounting to 425–475 dollars a year, and is renewable for a maximum period of four years. Further particulars can be obtained from the Cultural Relations Officer at the American Embassy, 5 Grosvenor Square, London, W.1, or from Dr. J. F. Foster, secretary of the Association of Universities of the British Commonwealth, 5 Gordon Square, London, W.C.1, with whom applications must be lodged in triplicate by May 1.

The sixth annual conference of the Stress Analysis Group of the Institute of Physics will be held during April 16–18 at University College, Gower Street, London, W.C.1. Prof. A. G. Pugsley, of the University of Bristol, will give the opening address on "The Aims of Stress Analysis". Further details can be obtained from the honorary secretary of the Group, E. K. Frankl, Engineering Laboratory, Cambridge.

A symposium on new research techniques in physics will be held in Rio de Janeiro, São José dos Campos and São Paulo, during July 15–29 under the joint sponsorship of the Brazilian Academy of Sciences, the National Research Council of Brazil and the Unesco Science Co-operation Centre for Latin America. Physicists from countries outside Latin America are invited to attend this symposium. Further information can be obtained from Prof. J. Leite Lopes, Centro Brasileiro de Pesquisas Fisicas, Avenida Pasteur 250, Rio de Janeiro.

A symposium on "Electron Transfer and Isotopic Reactions", sponsored jointly by the Division of Physical and Inorganic Chemistry of the American Chemical Society and the Division of Chemical Physics of the American Physical Society, will be held during June 11–13 at the University of Notre Dame. The chairmen will be Dr. F. A. Long, of Cornell University, and Dr. John R. Platt, of the University of Chicago. Further information can be obtained from Dr. James V. Quagliano, Department of Chemistry, University of Notre Dame, Notre Dame, Ind.; registration forms should be returned before May 31.

AN ELEMENTARY DERIVATION OF EDDINGTON'S NUMBER 10^{78}

By Dr. G. J. WHITROW
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It is well known that in Eddington's theory of the constants of Nature an important part is played by the number N, which he called "the total number of particles in the universe". Although in the final version of his theory[1] he assigned a precise value to N, it is still generally referred to by its order of magnitude[2], namely, 10^{78} (or sometimes 10^{79}). Eddington claimed that the precise value of N and also the integral value 137 for the fine-structure constant were associated with precise values, obtained by theoretical calculation, of the purely numerical constants of Nature. Although this claim depends on a subtle and intricate analysis of the concept of physical 'existence' which has not so far been generally accepted, the significance of a number of the order of magnitude of N is suggested by other theories as well. The equivalent mass of 10^{78} or 10^{79} nucleons is about 10^{55} gm., and a mass of this order of magnitude occurs in otherwise widely different theories of cosmology[3].

Although it is unlikely that there is any elementary means of deriving precise relations between the constants of Nature, it should be possible to obtain a simpler derivation than those previously given of the order of magnitude of N. It is the object of the present article to effect such a derivation.

In 1946, I showed[4] that the relation between the mass M and radius R of a homogeneous spherical world-model, defined so that the magnitude of its (negative) Newtonian self-gravitational potential energy was equal to its Einstein inertial energy Mc^2, was almost identical with the corresponding relation characterizing Einstein's static universe. A similar result has also been obtained independently by P. Jordan[5], following an idea due originally to A. Haas that the total energy of the world-model is zero.

In 1950, I suggested[6] that there are definite upper and lower limits to the range of photon energy, the upper limit being comparable with the inertial energy of a nucleon and the lower limit with an inertial energy corresponding to a rest-mass of the order 10^{-63} gm. I shall make use of these ideas here.

First, we impose the condition that the inertial energy of the world-model is equal to the absolute value of its gravitational energy (algebraic sum of the inertial and gravitational energy zero). The order of magnitude of the gravitational energy is GM^2/R. In the case of a Newtonian system in Euclidean space, the precise formula involves an additional factor 3/5 which we will here neglect. Consequently, the first equation governing our model is:

$$Mc^2 = \frac{GM^2}{R}. \tag{1}$$

We will next suppose that this model is only approximately homogeneous, being composed of N nucleons each of mass m and radius r regularly distributed. In Eddington's model, there are $\frac{1}{2}N$ protons and $\frac{1}{2}N$ electrons, the whole system being electrically neutral. So far as mass is concerned, we can neglect the electrons, and as we are interested only in order of magnitude we can replace $\frac{1}{2}N$ by N. If we assign to the nucleon a radius r such that the associated quantum of energy hc/r is equal to the inertial energy of the nucleon, we have

$$mc^2 = \frac{hc}{r}. \tag{2}$$

Regarding r as the minimum length in our model, it follows that hc/r is the maximum unit of photon energy. Similarly, the minimum unit will be given by hc/R. Previously, I identified this with the *inertial* energy of a mass (of the order of 10^{-63} gm.) which I tentatively suggested might be that of the neutrino[6], but which Mme. M. A. Tonnellat[7], following L. de Broglie[8], has recently suggested may be the finite (non-zero) proper mass of the photon. Without prejudice to these possibilities, and concentrating on the present model which is defined solely as an assemblage of nucleons, I now explore the consequences of identifying this smallest unit of energy with the absolute value of the self-gravitational energy of the nucleon, so that

$$\frac{Gm^2}{r} = \frac{hc}{R}. \tag{3}$$

Although the gravitational energy will not be given exactly by the expression on the left-hand side of (3), it will be of this order of magnitude, and as we are here interested only in the order of magnitude of R, there is no loss in assuming the numerical factor which ought to appear in (3) to be unity.

By way of justification of equation (3), we note that from the masses and lengths at our disposal, namely, M, m and R, r respectively, we can form six 'units' of energy.

Inertial energy	Mc^2	mc^2
Gravitational energy*	GM^2/R	Gm^2/r
Quantum energy	hc/R	hc/r

* Sign reversed and order of magnitude only.

Equation (1) identifies the first two items in the column on the left, and equation (2) identifies the first and third items in the column on the right. The justification of (1) is its inherent plausibility and the fact that it leads to a relation similar to that governing the Einstein static universe. The justification of (2) is that it leads to an order of magnitude formula for the radius of the proton which agrees with experiment. The justification of (3) is that it forms a bridge between the 'macroscopic' and the 'microscopic' properties of the model by identifying the remaining pair of 'units' which have not so far appeared in our formulæ.

We can eliminate h from equations (2) and (3) to get

$$\frac{Gm^2}{r^2} = \frac{mc^2}{R}, \tag{4}$$

whence, using equation (1), we deduce that

$$\frac{Gm}{r^2} = \frac{GM}{R^2}. \tag{5}$$

Since $M = Nm$, it follows immediately from (5) that

$$R = (\sqrt{N})r. \tag{6}$$

Substituting for R in equation (4) we deduce that

$$\frac{mc^2}{(Gm^2/r)} = \sqrt{N}, \tag{7}$$

that is, $\sqrt{N}$ is given by the ratio of the inertial to the gravitational self-energy (with sign reversed) of the nucleon. Consequently,

$$\sqrt{N} = \left(\frac{c^2}{G}\right)\left(\frac{r}{m}\right). \tag{8}$$

Substituting the empirical values for c, G and for r, m, we find that

$$\sqrt{N} \sim 10^{39}, \tag{9}$$

whence $N \sim 10^{78}$ and so M is of the order of magnitude of 10^{55} gm. Moreover, since r is about 10^{-13} cm., it follows from (6) that R is of the order of 10^{27} cm., that is, of the order of a thousand million light-years. So far as orders of magnitude are concerned, these are the respective values assigned by Eddington to the mass and radius of the uranoid (Einstein universe).

In order to derive equation (8), which does not involve h, we have appealed to equations (2) and (3) which contain h. In the course of derivation, equation (5) has been isolated. This equation, which is also free from h, can be given the following tentative physical interpretation. The expression Gm/r^2 measures the intensity of the self-gravitational field of a nucleon at its surface. If we can interpret the expression GM/R^2 as a measure of the intensity of 'world'-gravitation, we could think of the surface of the nucleon as a region of equilibrium between a field of intensity GM/R^2 directed radially outwards and a field of intensity Gm/r^2 directed radially inwards. Equation (5) would then be the condition that the model is in a gravitationally steady state. This equilibrium, however, would be unstable. For suppose R were to increase slightly in relation to r. This would be equivalent to a slight decrease in r, the radius of each nucleon, in terms of R. The intensity of inward gravitation at the surface of each nucleon would then exceed that of outward world-gravitation, and the nucleon would continue to contract further; that is, in terms of the radius of the nucleon as unit of length, the radius R of the whole model would continue to increase. Similarly, if R were to begin to decrease slightly in relation to r, it would continue to do so. Hence, the model would be unstable, and once disturbed would either expand or contract. This result follows without appeal to any peculiar force of 'cosmical repulsion'.

Finally, the present method can be adapted to determine the order of magnitude of the ratio of the electrostatic to the gravitational force between a proton and an electron. The electrostatic self-energy of a charge e of radius r will be comparable with the quantity e^2/r. If e is defined as the charge on the proton of mass m, then we can define another mass m' such that its inertial self-energy $m'c^2$ is equal to this electrostatic self-energy. Then,

$$\frac{e^2}{r} \sim m'c^2 = \left(\frac{mc^2}{Gm^2/r}\right)\frac{Gmm'}{r} = (\sqrt{N})\frac{Gmm'}{r},$$

from equation (7). Consequently, so far as order of magnitude is concerned,

$$\frac{e^2}{Gmm'} = \sqrt{N}. \tag{10}$$

Hence, if we regard m' as the mass of the electron of charge $-e$, we find that the order of magnitude of the ratio of the electrostatic to the gravitational force between a proton and electron is that of $\sqrt{N}$, namely, 10^{39}. It is well known that this result is empirically correct. The fact that the inertia of an electron is not regarded as entirely due to its electric charge makes no difference to this order of magnitude.

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CHEMICAL NATURE OF THE SECONDARY HYDROGEN PEROXIDE COMPOUND FORMED BY CYTOCHROME-*C* PEROXIDASE AND HORSERADISH PEROXIDASE

By Dr. PHILIP GEORGE

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FURTHER progress in understanding the specificity of the hæmoprotein enzymes, peroxidase and catalase, depends on finding out the structure of the intermediate compounds they form with hydrogen peroxide or alkyl hydroperoxides. In these reactions they differ most from methæmoglobin and metmyoglobin, which in other respects they resemble very closely[1].

Horseradish peroxidase forms three distinct intermediates with hydrogen peroxide which are interrelated as follows[2–5]:

$$\underset{\text{(brown)}}{\text{Horseradish peroxidase}} + \underset{1:1\text{ ratio}}{H_2O_2} \xrightarrow[\text{rapid}]{\text{very}} \underset{\text{(green)}}{\text{Compound I}}$$

$$\xrightarrow[\text{decomp.}]{\text{spontaneous}} \underset{\text{(pale red)}}{\text{Compound II}} \xrightarrow[H_2O_2]{\text{excess}} \underset{\text{(deep red)}}{\text{Compound III.}}$$

The intermediates are called compounds here rather than complexes, for the term complex implies that H_2O_2 or O_2H^- is a component part of the structure, and the experiments reported below do not support this generally held view[4,6,7].

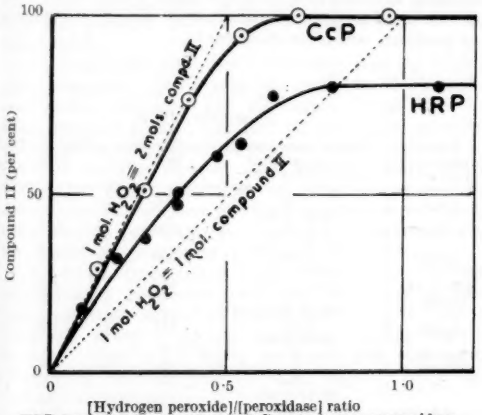
The horseradish peroxidase used had been prepared by Theorell's method and concentrations were calculated on the basis ε_{mM} at 402 mμ = 81 cm.$^{-1}$. Addition of hydrogen peroxide did not convert the enzyme completely into compound II, and a little of compound I remained. The increment of optical density at 425 mμ for complete conversion was obtained as $\Delta d_{425} = 0{\cdot}044$ cm.$^{-1}/\mu M$ by the addition of a little *p*-cresol, because reducing agents speed the conversion[5,8]. The cytochrome-*c* peroxidase had been prepared according to Abrams, Altschul and Hogness[6]: a spectrophotometric study of the formation of its cyanide complex showed that concentrations obtained from their value[7] of ε_{mM} at 408 mμ = 93 cm.$^{-1}$ were about 8 per cent too high due to

coloured impurities in the present sample. This factor was used in the calculations, and Δd_{425} for its conversion into compound II was taken as 0·0365 cm.$^{-1}/\mu M$, on the assumption that the bands of compound II and free enzyme should be equally intense as was observed with horseradish peroxidase.

The secondary compounds of horseradish peroxidase and cytochrome-*c* peroxidase were found to be rapidly reduced to the free enzyme by ferrocyanide ions in phosphate buffer at *p*H 5·4. A spectrophotometric titration of cytochrome-*c* peroxidase showed that 1·07 ± 0·08 mole compound II is equivalent to 1 mole ferrocyanide. With horseradish peroxidase the stoichiometric factor was 0·92 ± 0·08; this is a lower limit, because ferrocyanide may also react with the small amount of compound I present, which could be about 10 per cent of the total concentration of horseradish peroxidase. Compound II of cytochrome-*c* peroxidase was also titrated with ferrocytochrome-*c* and compound II of horseradish peroxidase with ferrous ions, stoichiometric factors near unity being obtained in each case.

This observation makes it easier to understand the results obtained in titrations of the enzymes with hydrogen peroxide, which gave the surprising result, shown in the accompanying graph, that more than one mole of compound II can be formed from 1 mole of hydrogen peroxide, the figure approaching 2 at low values of the hydrogen peroxide/peroxidase concentration ratio. The incomplete conversion of horseradish peroxidase into compound II with hydrogen peroxide alone is also shown in the graph. Earlier titrations[7] gave 1 mole compound II equivalent to 1 mole hydrogen peroxide. No explanation could be found to explain the disagreement of the results, which appears to be outside any experimental uncertainties—for example, the exact value for $\Delta d_{425}/\mu M$; but the earlier workers used a much higher enzyme concentration that may have contained a donor. The results of the ferrocyanide titration and peroxide titration are nevertheless consistent on a stoichiometric basis.

Since compound II has only one oxidizing equivalent compared to two originally present in hydrogen peroxide, the latter or O_2H^- cannot be component parts of the structure. The following oxidizing agents, hypochlorous acid, hypobromous acid, bromate, periodate, chlorite, chlorine dioxide, ozone and perdisulphate with silver ions were all found to give an intermediate compound with horseradish peroxidase spectroscopically indistinguishable from that obtained with hydrogen peroxide or methyl hydroperoxide. Cytochrome-*c* peroxidase gave its compound II with all but hypochlorous and hypobromous acids, which caused enzyme destruction. There is some evidence, too, that compound I is also formed in these systems. It is possible that a peroxide formed from the oxidizing agent might act as an intermediate; but in the case of hypochlorous acid, hypobromous acid, chlorite and chlorine dioxide, enzyme activities obtained with guaiacol compared favourably with those obtained using hydrogen peroxide, which suggests direct action between peroxidase and oxidizing agent in these cases.



Titration of cytochrome-*c* and horseradish peroxidase with H_2O_2 in 0·01 *M* phosphate buffer, *p*H 5·4, at 30° C. Percentage complex formation was determined from optical density measurements at 420, 425, 430 and 435 mμ. The enzyme concentrations used were 1·08–1·11 μ*M* horseradish peroxidase and 0·24 μ*M* cytochrome-*c* peroxidase in 1-cm. and 5-cm. cuvettes respectively

The discovery that compound II has one oxidizing equivalent resolves the difficulty in formulating its reaction with reduced cytochrome-*c* on a two-equivalent basis[8]. Similarly, it is possible to explain Chance's observation that the transition from compound I to compound II with horseradish peroxidase is speeded up in the presence of donors[9], for the transition would be a simple reduction process:

Compound I + AH_2 → Compound II + AH +, etc.

I have discussed this essentially stepwise mechanism in a recent review[10].

Since compound II is one oxidizing equivalent higher than the trivalent ferric state of the enzyme, it should be regarded as a compound of quadrivalent iron; and in this respect it is similar to ferrimyoglobin–hydrogen peroxide compound which recent experiments of George and Irvine[11] have also shown to possess one oxidizing equivalent. At least four types of structure appear possible for these compounds: (1) a simple quadrivalent iron structure containing Fe^{4+}; (2) a derivative[11] such as the ferryl ion, FeO^{2+}; (3) a diradical structure in which trivalent iron forms 'one end of the radical', the other end being a normal radical grouping at a methene carbon atom, a pyrollic carbon atom, or some other atom within the conjugated network of porphyrin ring and hæmatin-protein linkage; (4) a higher oxidation state of the hæmatin group in which the electron has been removed from a π-orbital common to the ring as a whole as in the case of the metal phthalocyanines[12].

This investigation was carried out in the Eldridge Reeves Johnson Foundation for Medical Physics, Philadelphia, during June–December 1951, while holding a Rockefeller Foundation Fellowship, and was an extension of the earlier work on metmyoglobin carried out in Cambridge[11]. [Feb. 22.

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THROMBIN AND THROMBIN-PROTEASE

By H. KOWARZYK

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A SERIES of investigations carried out in our laboratories strongly supports the proteolytic nature of the clotting reaction discussed in earlier communications in *Nature*[1,2].

A convenient method of determination of non-protein nitrogen in serum or clotting plasma was devised[3] which consists essentially in removing the urea and in precipitating the proteins with the Pincussen reagent[4]. In order to facilitate the release of non-protein nitrogen into the supernatant, the clotting materials were incubated in tubes filled with glass beads; the resulting clot-films could easily be stirred. With this method it was found[5] that the non-protein nitrogen content of human blood serum markedly exceeds that of plasma, averaging 1·4 mgm. per cent nitrogen (ranging from 0·5 to 2·4 mgm. per cent).

We have found[6] that clotting of recalcified oxalate plasma of different species results in a similar rise in the non-protein nitrogen. Also the clotting of eu-globulin solutions obtained from diluted plasma by acidification and containing all the factors involved in the clotting except calcium ions gives a rise of non-protein nitrogen after proper recalcification[7]. The results are shown in Table 1.

The increase of non-protein nitrogen in the clotted plasma does not correspond with its prothrombin content or serum accelerator. It does not correspond in the clotted eu-globulin solutions with their fibrinolysin content. Thromboplastin prepared from brain, placenta or human milk does not yield a further appreciable increase of non-protein nitrogen, though a slight proteolytic power was detectable in these preparations. The rise of non-protein nitrogen in the eu-globulin solutions is the same at different temperatures from 6° to 37° C. provided the clot-forming is complete. Thrombin preparations obtained from human or bovine plasma without addition of tissue thromboplastin clot the oxalate plasma, the eu-globulin and highly purified prothrombin-free fibrinogen solutions, giving roughly the same increase of non-protein nitrogen per 100 mgm. fibrinogen[8,9] (see Table 2). The same fibrinogen solutions gave an identical rise of non-protein nitrogen if clotted with thrombin preparations of human, hen and carp origin.

Table 2. Rise of Non-Protein Nitrogen per 100 c.c. Plasma or 100 mgm. Fibrinogen (in mgm.)

Clotting substrate \ Thrombin	Human	Bovine
Human plasma	0·51 (0·27–0·73)	0·31 (0·02–0·54)
Eu-globulins from human plasma	0·17 (0·11–0·24)	0·23 (0·02–0·38)
Human fibrinogen	0·18 (0·08–0·33)	0·28 (0·02–0·53)
Bovine fibrinogen	0·16 (0·08–0·30)	0·22 (0·14–0·30)

If oxalate plasma is added to fresh serum the additional non-protein nitrogen appears just as with thrombin, provided that the added serum suffices for clot formation; there is no increase of non-protein nitrogen if no clot is formed.

Pre-adsorption of plasma with barium carbonate inhibits the increase of non-protein nitrogen after recalcification if the prothrombin is really exhausted. If highly purified prothrombin preparations are added to such a plasma, the well-known rise of non-protein nitrogen is re-established together with the clot formation after recalcification[10] (Table 3).

Highly purified prothrombin-free fibrinogen preparations give no rise of non-protein nitrogen if incubated at 20° C. for 1 hr. Addition of prothrombin to such a solution does not cause any rise in non-protein nitrogen if no thrombin is formed, that is, if the preparations are of highest purity. The non-protein nitrogen difference found in this series of ten experiments amounted on the average to 0·03 mgm. per 100 mgm. fibrinogen. The increase reappears together with clot formation if prothrombin is added which contains traces of thrombin, or if the fibrinogen-prothrombin system develops some traces of thrombin[11,12]. The increase of non-protein nitrogen is proportional to the fibrinogen content of the clotting systems and does not vary with the different amounts of added thrombin, provided it is sufficient to perform complete clotting[10].

Table 1

Species	Oxalate plasma			Eu-globulin solution		
	Rise of non-protein nitrogen after recalcification*	Prothrombin in units (two-stage)	Serum accelerator (units)††	Rise of non-protein nitrogen after recalcification†	Clotting time (sec.)	Fibrinolysin index
Dog	1·78	324	2,900	0·43 (0·23–0·60)	100	9·0
Goat	?	370	c. 18,500	0·36 (0·12–0·65)	160	0·5
Pig	?	310	5,000	0·42 (0·25–0·56)	150	1·5
Man	0·55 ± 0·32	270 ± 27	470 ± 140	0·19 ± 0·08	540	1·0
Rabbit	1·28 ± 0·67	160	3,000	0·14 (0·05–0·25)	860	0·1
Horse	1·39	410	2,100	0·11 (0·05–0·17)	615	1·0
Guinea pig	1·93	200	2,000	?	?	?
Rat	1·40	150	2,800	?	?	?
Hen	0·39	c. 80**	+	0·80 (0·31–1·34)	750	2·0
Carp (*Cyprinus carpio* L.)	?	c. 80**	+	0·56 (0·49–0·73)	260	0·1

* Non-protein nitrogen in mgm. per 100 c.c. plasma.

† Non-protein nitrogen in mgm. per 100 c.c. eu-globulins, corresponding to 100 c.c. plasma.

** Tested with homologous thromboplastin and on homologous prothrombin-free plasma as test-solution.

†† Tested on bovine prothrombin and thromboplastin from rabbit, hen or carp brain respectively.

Table 3. Rise of Non-Protein Nitrogen per 100 c.c. Plasma or 100 mgm. Fibrinogen (in mgm.)

Clotting substrate	Recalcification	Human thrombin added
Human plasma pre-adsorbed with barium carbonate but containing traces of prothrombin	0·36 ± 0·18	
Human plasma without detectable prothrombin	0·10 ± 0·27	0·31 ± 0·19
The same plasma with prothrombin preparation added	0·54 (0·32–0·70)	
Prothrombin-free fibrinogen	0·02 (from −0·01 to +0·05)	0·19 (0·10–0·30)
Crude fibrinogen with prothrombin added	0·16 (0·05–0·38)	

These observations led to the following conclusion[11]: "The protease causing this increase of non-protein nitrogen is activated under strictly the same conditions as are essential for the conversion of prothrombin to thrombin". The simplest conclusion, that the thrombin-protease is identical with thrombin, was not drawn from this statement, though it was much discussed in our laboratory. The coincidence of rise of non-protein nitrogen with clot formation, however, is not a definite proof of the identity of substances acting.

We have found the asymptotic time-curve regularly with thrombin preparations containing beyond any doubt a 'fibrinolysin'. The 'fibrinolysin' is not preformed in the eu-globulin solution, but in traces, which differ in preparations from different species. It is the addition of calcium ions which induces the activation of the fibrinolysin; this observation was made some time ago by Ferguson, Travis and Gerheim[13], who stated: "the protease impurity always shows up best in recalcified thrombin mixtures". This activation can be demonstrated without difficulty with eu-globulins from hen plasma. Lysis of the clot after recalcification takes 7 hr. (on the average); the same eu-globulin clotted with thrombin of hen plasma origin does not show the beginning of the lysis until after 96 hr.

Moreover, the rise of non-protein nitrogen in a fibrinogen solution (recalcified or clotted with a crude thrombin preparation) does not show the expected increase parallel to the visual appearance of the fibrinolysis.

In eu-globulin material it is only slightly higher after complete fibrinolysis (for example, 0·31 mgm. non-protein nitrogen per 100 mgm. fibrin after an incubation of 4 hr.); but nevertheless it is considerably smaller than the non-protein nitrogen found by Lorand[2] in the clotted, but not liquefied, fibrin material. These discrepancies must be caused by differences in the size of the degradation products, some being precipitated, the others not, by the Pincussen reagent or by Lorand's procedure.

The fibrinolysis effected by different fibrinolysin preparations (we have made observations on the preparation of Silva and Rimington[14] and on that of Kaulla[15]) gives a greater increase of non-protein nitrogen (0·93–1·17 mgm. nitrogen per 100 mgm. fibrinogen) and differs in this respect from the thrombin protease.

We agree we have no definite experimental evidence against the protease nature of thrombin; but we do not feel the results accumulated so far are adequate for this far-reaching assumption. We believe rather that our purest 'fibrinolysin-free' thrombin preparations contain a protease which is responsible for the asymptotic rise of non-protein nitrogen during clot formation. [July 25.

Note added in proof. In the meantime, we have succeeded in obtaining a powerful preparation of the thrombin-protease. This protease gives an asymptotic or roughly asymptotic time-curve of the non-protein nitrogen if added in traces to a concentrated fibrinogen solution; if added in excess to diluted fibrinogen solutions, it causes fibrinolysis and a high progressive rise of the non-protein nitrogen. This protease represents the active form of the relatively inert or inhibited fibrinolysin found by Guest and Ware in their purest thrombin preparations[16].

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STRUCTURE OF COMETS AND THE FORMATION OF TAILS

A PAPER on "The Structure of Comets and the Formation of Tails"[1] has been published by Dr. R. A. Lyttleton, which continues his investigations described in a previous paper, "On the Origin of Comets"[2]. It was suggested in the latter communication that comets were formed during the passage of the sun through a cloud of interstellar dust, the particles converging to the accretion axis where collisions destroyed their transverse velocities. In the more recent paper it is shown that the self-gravitation of comets is negligible in comparison with the differential force due to the sun, except at very great distances from the sun, and the particles composing them move in independent orbits.

As was shown in the earlier paper, during the perihelion passage the whole comet will turn inside out, particles on one side of the orbital plane crossing to the other, and contraction in the neighbourhood of perihelion perpendicular to the orbital plane should take place. Observational evidence supports this view because the nuclei of comets are actually known to contract considerably as the bodies approach perihelion and to expand again as they recede from the sun. Collisions between the particles will take place, and a rough estimate shows that for a comet of mass 10^{19} gm. about 10^{14} gm. would be involved in collisions at each return to perihelion. The relative velocities of the colliding particles depend on the orbital speed and the general size of the comet when it is at a great distance from the sun, but can be taken to be of the order $\frac{1}{4}$–$\frac{1}{2}$ km./sec., and such velocities are sufficient to pulverize the colliding

particles. As a result of these collisions a range of small-sized particles would be formed, and those with dimensions of the order of 10^{-5} cm. would be driven off from the comet by the sun's radiation pressure; in this way a comet would produce tails indefinitely at each return to perihelion.

One important effect may be mentioned. Differences of period in the particles must result in their distribution around the orbit of the comet, and in some cases these particles are responsible for meteor streams. In addition, collisions could lead to a resistance which would have effects similar to the Poynting–Robertson effect, though differing quantitatively, thus decreasing the perihelion distances of comets and accelerating their velocities.

This is described in a paper by H. P. Robertson, under the title, "Dynamical Effects of Radiation in the Solar System"[3]. Poynting showed that the absorption and re-radiation of solar radiation by small bodies produced a tangential drag, decreasing the angular momentum of the particles and causing them to spiral inwards towards the sun; and Robertson examined the problem from the point of view of relativity. He obtained expressions for the retarding force which differed from those of Poynting and Larmor, and he admitted that the values caused by the drag could not be reconciled at the same time between the various elements. Lyttleton suggests that the further mechanism provided by his theory, combined with the Poynting–Robertson effect, might partly solve the difficulty that Robertson met in dealing with Encke's comet. It is hoped that Lyttleton will pursue this important subject later.

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ELECTRONIC TELEPHONE EXCHANGES

A TELEPHONE exchange is basically a switch or aggregate of switches, by means of which two subscribers are connected together for the conduct of a telephone conversation. A manual exchange system is one in which human operators carry out the interconnexions between the subscribers; but the idea was conceived some sixty years ago that this work could be conducted automatically by means of electromagnetic switches operated by impulses sent by the subscriber initiating the telephone call. This conception has been developed notably in the past thirty years or so; and about half a dozen systems of automatic telephone exchange have achieved large-scale use in different parts of the world. During this period of development the thermionic valve amplifier, or repeater, has also been applied and has become a normal piece of equipment in any trunk or long-line telephone system. More recently, attention has been given in Great Britain and other countries to the possibilities of automatic telephone exchanges constructed with electronic switches, in order to determine whether exchanges cheaper and more reliable than the present types using electro-mechanical switches may thereby be produced.

At a meeting of the Institution of Electrical Engineers on March 13, a very interesting paper entitled "Electronic Telephone Exchanges" was read by T. H. Flowers, of the Post Office Research Station, Dollis Hill. The author approached his subject in a broad manner, and did not merely limit his theme to a simple problem of replacing a number of electro-mechanical switches by the corresponding valve assemblies carrying out the same individual functions. The paper considered the problem of line interconnexion anew from first principles, and then discussed some of the practical forms which electronic exchanges might take, the possibilities and characteristics of such exchanges, and—very briefly—the questions of cost and reliability. The multiplex switching arrangements required can make use of carrier-frequency or pulse techniques to obtain frequency- or time-division systems respectively. At the reading of the paper a demonstration was given of the operation of a model three-line exchange using trains of pulses for interconnexion, line-signalling and speech transmission between the subscribers' installations. Speech frequencies of 300–4,000 c./s. can be transmitted with a pulse-repetition frequency of 10 kc./s. for the trains of pulses. For a switch dealing with a hundred channels, the time-spacing between the channels is then 1 μsec., and a pulse width of 0·3 μsec. is desirable. As Mr. Flowers points out in his paper, transmission gain and loss, variation of loss, and distortion are features of electronic connector switches not found in existing types using metal-to-metal contacts; but, on the whole, better speech transmission could be given by electronic systems using the connector switches described in the paper.

In the discussion, which followed the reading of the paper, several speakers emphasized the need for caution before too hurriedly adopting these new techniques, since cost and effective life are major items for consideration. A modern telephone exchange is expected to last for at least twenty-five years, and, as Mr. Flowers pointed out, it would be necessary to guarantee at least a five-year life for the components, including the valves, if the effect of failures on the service and the cost of replacing faulty parts are not to be prohibitive. The meeting provided a very useful introductory outline on the possibilities of electronic automatic telephone exchanges, and, whatever may be the outcome, there appears to be enough encouraging evidence to justify proceeding with the development of such exchanges.

COSMIC-RAY EXPEDITION TO THE HIMALAYAS

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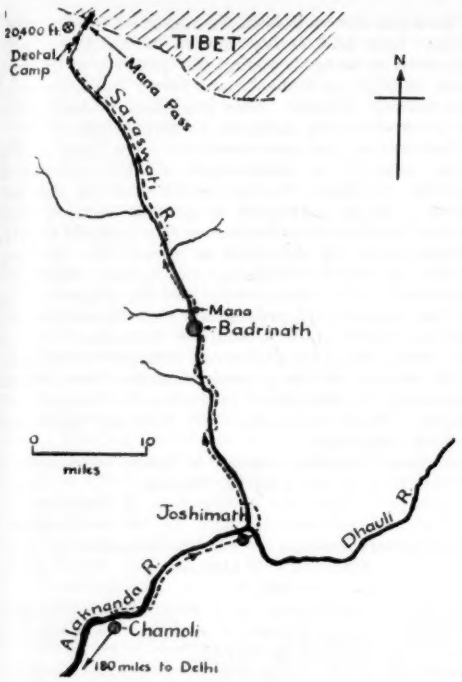
THE University of Ceylon recently sponsored a cosmic-ray expedition to the Garhwal Himalayas. Early in 1951 various high-altitude experiments were designed by Dr. V. Appapillai and me, and a grant from the research committee of the University was obtained to enable these to be carried out. Accounts of the experimental work will be published in due course as the results become available. The purpose of the present note is to communicate some general information concerning the expedition, which, it is felt, will be of interest to cosmic-ray physicists and perhaps to others.



The scientific equipment was limited to photographic plates of the nuclear-research type. There is no doubt that electronic equipment could have been taken over the entire route; but the much greater weight to be transported (due particularly to the necessity for carrying a diesel-electric set) was not within the scope of our budget. Approximately two hundred plates were taken, some of them in aluminium boxes evacuated before leaving Colombo to minimize fading, and the remainder in air. These were placed at various heights up to 18,400 ft. (The highest point, 19,150 ft., was reached by the expedition on the return trip to recover the plates; during the first ascent we were hampered by a snowfall and were forced to place the plates in the valley rather than on the peaks adjacent to it.) The exposure time was fifteen days at the maximum height, and longer for lower altitudes. During the period of exposure the expedition returned to Badrinath at 10,400 ft.

Garhwal, a district of the Kumaun Division of the United Provinces, is bordered on the north by Tibet. The site selected for making cosmic-ray exposures was the Mana Pass, one of the highest passes between India and Tibet. It is situated at approximately lat. 31° 5′ N., long. 79° 26′ E., and has an altitude of 18,400 ft. at its highest point. The Pass is accessible by a 28-mile goat trail from the town of Badrinath (see map), which is in turn linked by a 56-mile footpath, maintained by the Public Works Department, to Chamoli, which is the terminus of the bus route from the rail-head at Kotdwara.

Badrinath is one of the holy places of the Hindu religion and is visited annually by a large number of pilgrims. There is, therefore, ample catering and accommodation available, and, as the town itself is at 10,400 ft. in the heart of high country, it provides an excellent base for sallies among the surrounding mountains. The actual route followed by the expedition is indicated by the dotted line on the map. There are no villages up the Saraswati valley beyond Mana; but there are camping sites used by Tibetan shepherds and traders who cross the Pass with their flocks during the season. From Badrinath to the Pass the trail climbs steadily and requires about four days.

Coolies were drawn from Chamoli for transport to Badrinath. These men are Dotials, well built and willing, and carry more than 80 lb. for Rs. 3 or 4 a day plus an issue of rice. However, they are unaccustomed to extreme cold, and we replaced them by Bhotias from Mana village, two miles beyond Badrinath. These carried 60-lb. loads to the highest camp-site (Deotal, 17,900 ft.).

There has been a considerable amount of discussion concerning suitable sites for high-altitude laboratories, and the Himalayas seem to have received less than their merited share of attention. Certainly they have attracted all too few scientific expeditions. One of the incidental aims of our expedition was to explore the possibilities of this area of the Himalayas for serious cosmic-ray work. We felt that the average physicist is something less of a mountaineer and is unlikely to conduct prolonged dispassionate investigations while clinging on a crumbling crag. Important requirements of a good site are accessibility and safety as well as high altitude.

After correspondence with a number of Indian physicists, surveyors and government officials, we concluded that Sikkim and Garhwal contain the most promising sites either for an expedition or a semi-permanent cosmic-ray station. Both offer reasonable access to heights around twenty thousand feet without demanding special mountaineering skill or imposing undue hazards. Sikkim is at present closed to travellers, and hence Garhwal, in our opinion, offers the best opportunity.

The site adopted by us may seem remote, being some eighty-four miles by foot or pony from the bus terminus; but it should be noted that the general level of land rises as one proceeds farther into the Himalayas, facilitating the attainment of a high altitude. For example, the head-waters of the Saraswati River are in the valley adjacent to the Pass, that is, at about eighteen thousand feet. Moreover, the snow line rises as the general altitude advances. During the summer season, the Pass is free from snow.

While making the approach-marches up the Saraswati valley we observed at least half a dozen flat-topped peaks easily accessible by gentle slopes from the trail, and all estimated at well over seventeen thousand feet. Immediately west of the Pass itself, and within a mile of the Tibet border, is a peak rising to 20,400 ft. with a spur to the east at 19,150 ft. Both can be climbed readily, had only a light snow covering even late in October and would present no serious obstacle to laden coolies.

The caution must be added, however, that in Garhwal the choice of season is very important. Our expedition was there during September and October, and it is our opinion that some two months earlier would have been preferable. The region is then much wetter but free from snow. By November the Pass is closed and Badrinath deserted until April of the following year.

We acknowledge our thanks to Mr. M. A. Straus, of the University of Ceylon, whose experience in Colorado, United States, made him invaluable in the planning and conduct of the expedition.

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

Radiochemical Studies on Free Radicals

When the first attempt was made to determine the coefficient of accommodation of free organic radicals, a 'guard mirror' of lead was placed in front of a second mirror; by gradually reducing the length of the guard mirror until the second mirror was attacked, it was possible to show with a fair degree of accuracy that free radicals reacted with a lead mirror at every first impact. On a clean glass wall no more than about one in a thousand radicals was not reflected[1].

We have tried to simplify, and at the same time to increase the accuracy of these experiments by a radiochemical technique. The use of radioactive mirrors in free radical studies is not new. Leighton and Mortensen[2] employed invisible mirrors of radium D and E to detect very low concentrations of free radicals produced by photolysis. Using a β-electroscope, they were unable to measure directly the change in activity of the mirror; but they could trap and measure the radioactive products. The advantages of this radioactive mirror method over the visual observations were doubtful, so that in later studies the authors returned to the old method. We have improved the radioactive technique by using a Geiger counter instead of an electroscope, and a 'slit' method for measuring the activity of the mirror.

A lead block (6 cm. × 6 cm., of 1 cm. thickness) was provided in the middle with a slit of adjustable width, and with a groove on the under side, so that, placed on the reaction tube, it could be moved smoothly along its length. A small Geiger tube was fitted on top of the slit, which was at right angles to the groove. The aperture could be narrowed to less than 1 mm., thus making it possible to measure small cross-sections of the mirror. With this arrangement the distribution of radioactivity along the mirror could be measured by taking sectional counts. The extent of change in this distribution due to the action of free radicals on one mirror gives immediately the length of mirror necessary to reduce the concentration of free radicals to zero, thus making the runs with guard mirrors of different lengths unnecessary.

The first indicator to be tried was antimony-124; but, because of its penetrating γ-radiation, sharp definition could not be obtained. Radium E was then chosen, because of its pure β-emission; it proved to be quite suitable. Unlike Leighton and Mortensen, we separated the radium E electrochemically and used it as the indicator for a heavy bismuth mirror. This ensures that throughout the experiments the conditions offered to the impinging radicals remain the same.

With this device it was easy to determine exactly the limit which ethyl radicals reached on the mirror. It was gratifying to find with the bismuth mirrors a full confirmation of the result obtained by Paneth and Herzfeld with lead mirrors and a different technique, namely, an accommodation coefficient of 1 for the impact of the radicals on the metal[3]. A further advantage of the method is that the number of free radicals reacting can be determined without much difficulty. As we know now that with a suitable mirror every radical is combining, it is only necessary to measure the radioactivity removed from a sufficiently long and heavy mirror of known specific radioactivity in order to find the quantity of bismuth ethyl ($Bi(C_2H_5)_3$) formed, and hence the number of free radicals present. This procedure is easier than the microchemical methods hitherto employed[4,5].

Radioactive indicator methods have been shown, or promise, to be useful, also in other free radical studies. Durham, Martin and Sutton, in this laboratory, have developed a general radiochemical method for the identification of free radicals and the determination of the yield in which they are produced[6]. It seems promising, too, to use radioactive indicators for the study of the half-life of free radicals in solution, although preliminary experiments indicate that a much more elaborate technique will be necessary. Also, the still more difficult investigation of the electric and magnetic moments of free radicals may well be undertaken by radioactive tracer techniques. Experiments on these lines are under way in this laboratory.

A more detailed report is being submitted to *Transactions of the Faraday Society*.

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Jan. 7.

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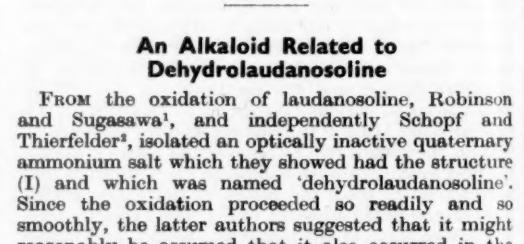
An Alkaloid Related to Dehydrolaudanosoline

From the oxidation of laudanosoline, Robinson and Sugasawa[1], and independently Schopf and Thierfelder[2], isolated an optically inactive quaternary ammonium salt which they showed had the structure (I) and which was named 'dehydrolaudanosoline'. Since the oxidation proceeded so readily and so smoothly, the latter authors suggested that it might reasonably be assumed that it also occurred in the plant cell, but they were unable to cite any alkaloid which was, or could be, a derivative of (I). Later, Folkers, Koniuszy and Shavel[3] ascribed to certain alkaloids of *Eyrthrina* species structures based on the ring system of (I); but recent work of Carmack, McKusick and Prelog[4] has shown that other structures are more likely.

However, a simple derivative of (I) has now been obtained from a natural source. From the bark of *Cryptocarya bowiei* (Hook.) Druce, collected in northern Queensland, a water-soluble alkaloid,



I II

$C_{20}H_{21}O_4NI$, melting point 214° C. (decomp.), $(\alpha)_D^{21°}$ −151° (water) was isolated in about 0·7 per cent yield. It contained three methoxyl groups, and on methylation gave an O-methyl ether, $C_{21}H_{23}O_4NI$, melting point 153–155° C., $(\alpha)_D^{21°}$ −175° (water), which alkali converted to methine-*A*, $C_{21}H_{23}O_4N$, melting point 102° C., $(\alpha)_D^{20°}$ −221° (chloroform). Further methylation and alkaline degradation eventually gave optically inactive methine-*B*, $C_{22}H_{27}O_4N$, melting point 111° C., which was identical (mixed melting point) with an authentic specimen prepared in the same way from dehydrolaudanosoline iodide. The methiodide of methine-*B* was unaffected by alkali, but ozonolysis of methine-*B* gave 4,5-dimethoxyphthalaldehyde, characterized by conversion to the ethylimide of *m*-hemipinic acid. The expected 6-dimethylaminoveratric aldehyde could not be isolated from this ozonolysis; but it was obtained together with the known 6-ethylveratric aldehyde from dihydromethine-*B*, $C_{22}H_{29}O_4N$, melting point 120° C. Further, pyrolysis of the O-methyl alkaloid chloride gave the indole derivative (II), melting point and mixed melting point 199° C.

Bark collected in southern Queensland appeared to contain none of the alkaloid described above, but yielded (1·5 per cent) a different alkaloid $C_{19}H_{20}O_4NI$, melting point 246° C., $(\alpha)_D^{2·°}$ −186° (water), which had one methoxyl, one methylenedioxy and one hydroxyl group (O-methyl ether, $C_{20}H_{22}O_4NI$, melting point 227° C., $(\alpha)_D^{21°}$ −179° (water)). It is probable that this second alkaloid also is a derivative of (I).

Further work on the structures and occurrence (L. J. Webb, Commonwealth Scientific and Industrial Research Organization) of these alkaloids is in progress.

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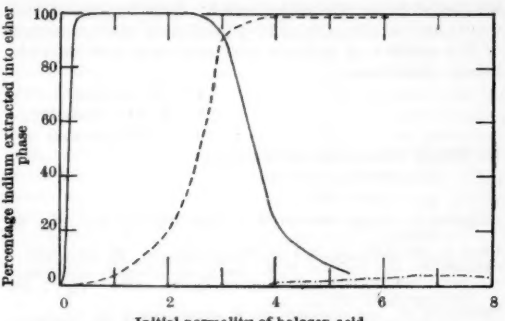
Solvent Extraction of Indium

THE separation and determination of small amounts of indium in the presence of other metals, especially beryllium and iron, are analytical problems of importance for which no entirely satisfactory solution has as yet been published. Mathers and Prichard[1] precipitated iron as its cupferrate; but some 4 per cent of indium is co-precipitated. May and Hofmann[2] extracted indium together with some other metals as oxinates into chloroform, decomposed these complexes and then re-extracted indium with a mixture of cupferron and dithizone, neither reagent alone being entirely satisfactory; but this complicated procedure may lead to considerable losses of indium. It may be inferred that the problem was not solved in the Manhattan Project[3]. In the separation of fission products, indium has been precipitated with cadmium as its sulphide and then separated as its basic acetate[4], but the chemical yields are only 50–60 per cent. Milner[5] has recently described a procedure in which indium and iron are separated from beryllium by extraction into chloroform as oxinates. After decomposing these oxinates, ferric chloride is extracted into ether from concentrated hydrochloric acid, and the residual indium determined polarographically.



Percentage of indium extracted by diethyl ether from halogen acids of various concentrations. For solutions in hydrochloric and hydriodic acids, 20 ml. aqueous phase was equilibrated with 20 ml. of fresh ether. With hydrobromic acid, 20 ml. of aqueous phase was shaken with 30 ml. of 'conditioned' ether (ref. 6)

The extractability of the chlorides and bromides of the trivalent elements aluminium, iron, gallium, indium and thallium by diethyl ether from the corresponding halogen acid shows certain trends[6], and if these trends are continued with the iodides, indium iodide may be expected to be quantitatively extractable. Ferrous chloride and bromide are extracted to a negligible extent as compared with the corresponding ferric halides; ferric iodide cannot exist in aqueous solution. Hence the possibility presents itself of separating indium completely from iron, the most troublesome interfering element, in a single extraction process.

In agreement with expectation, the percentage of indium halide extracted by diethyl ether from the corresponding halogen acid was found to increase in the order Cl < Br < I, and the maximum extraction (which reaches 100 per cent in the case of the iodide) occurs at decreasing acid concentrations in the same order, as shown in the accompanying graph. Indium may thus be extracted quantitatively as its iodide from hydriodic acid of normalities between 0·5 and 2·5, the distribution coefficient being greatest at 1·5 *N*. The presence of 0·5 *M* fluoride, chloride, bromide, cyanide, phosphate or citrate ions does not interfere in any way. Moreover, iron and beryllium remain quantitatively in the aqueous phase. It is a point of considerable analytical convenience that the extraction may be effected equally well from sulphuric acid of normalities ranging from 0·5 to at least 7·0 *N* to which the appropriate amount of potassium iodide has been added, and in this way the use of unstable solutions of hydriodic acid is avoided.

The investigation was carried out with indium-114 at a concentration of 30 mgm./l. Radioindium has been used previously by Knox and Spinks[7] in a tentative study of the distribution of indium chloride between hydrochloric acid and various organic solvents. In the present work, type *M*.6 10-ml. Geiger–Müller liquid counters were employed and a novel routine was adopted to ensure that all solutions being counted had the same density and essentially the same composition, so that a common and serious

source of error was eliminated. Full details of this work, and of complementary studies of the extraction of the iodides of gallium and thallium, will be published elsewhere.

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Use of Ion Exchange Resins to determine the Charge of an Ion in Aqueous Solution

THE use of mono-functional ion-exchange resins with strongly acidic or basic groups provides an extremely simple and rapid technique for the determination of the ionic charge of an anion or cation in aqueous solution. The method has the further advantage of being applicable to solutions containing only very small concentrations of the unknown ion.

For any exchange reaction between two ions A and B of the same sign :

$$n_1.A^{n_2} + \overline{n_2B^{n_1}} \rightleftharpoons \overline{n_1.A^{n_2}} + n_2B^{n_1};$$

application of the law of mass action to the equilibrium leads to the expression :

$$K_c = \frac{[\overline{A^{n_2}}]^{n_1} \times [B^{n_1}]^{n_2}}{[A^{n_2}]^{n_1} \times [\overline{B^{n_1}}]^{n_2}}, \qquad (1)$$

where A is the anion or cation of unknown charge (n_2) and unknown concentration, and B is an ion of both known charge (n_1) and known concentration. The square brackets signify concentrations in some suitable units, and the bar above an ion indicates that it is in the resin phase.

As first noted by Kressman and Kitchener[1], an approximately constant value for K_c results if the 'concentrations' of the ions in the resin phase are represented by equivalent fractions and if no activity coefficient corrections are attempted with the ions in the aqueous phase. Concentrations in the aqueous phase may, for convenience, be expressed as equivalent concentrations. If X and $\bar{X}$ represent equivalent fractions of the ion A in the solution and resin phases, respectively, equation (1) may be written :

$$K_c = \frac{\bar{X}^{n_1} \cdot (1-X)^{n_2}}{(1-\bar{X})^{n_2} \cdot X^{n_1}} \cdot C_s^{n_2-n_1}, \qquad (2)$$

where C_s is the total concentration of exchanging ions in the aqueous phase, expressed as equivalents per litre.

Provided that the initial concentration of B in the solution is at least a hundred times greater than that of A, $(1-\bar{X})^{n_2}$ and $(1-X)^{n_2}$ each approximate to unity, and for C_s may be written C_B, the known concentration of ion B in solution, expressed in equivalents per litre. Let w be the weight of resin (initially all in the B^{n_1} ionic form) and $\bar{C}$ its capacity in equivalents per gram. If V is the volume of the solution in equilibrium with the resin and k_2 and k_1 are the values of some physically measurable property of A proportional to the number of molecules of A present in V litres of a solution before and after equilibration with the resin, then (2) may be expressed in the form :

$$\log \frac{k_1}{k_2-k_1} = (n_2-n_1+1) \log C_B + \log\left(\frac{V}{K_c.w.\bar{C}}\right). \qquad (3)$$

To determine n_2, a series of solutions is prepared, each solution containing approximately the same concentration of A but a different, known, concentration of B. After measuring values of k_2, the solutions are then equilibrated with the resin, filtered, and the corresponding values of k_1 found for the filtrates. (The exact concentrations of A corresponding to k_2 and k_1 need not be known.) Provided that V and w are kept constant in all experiments, the slope of the plot of $\log k_1/(k_2-k_1)$ against $\log C_B$ enables n_2 to be evaluated, as both the equivalent concentrations and the charge of B are known.

k_2 and k_1 may be optical densities of the solution, polarographic step-heights, counts per minute of a radioactive element suitably incorporated into A, etc. Absorptiometric or polarographic techniques may be used with concentrations of A between about 10^{-6} and 10^{-3} M, and the use of a radioactive 'tracer' extends the range down to 10^{-10} M or less.

C_B may generally be varied within the range 0·01–0·5 N; but the choice of B, its concentrations, and the ratio V/w are best determined empirically so as to give the most suitable values of k_1. The ratio V/w, however, must not be less than about 5–10, or errors due to variable water absorption by the resin used may become significant. Mechanical agitation for one to two hours at a controlled temperature generally ensures a sufficient approach to equilibrium between solution and resin phases.

Using both absorptiometric and radioactive tracer methods, the procedure outlined above has been successfully applied to the determination of the ionic charge of both anions and cations resulting from the co-ordination of 'transitional' elements with organic and other groups. Details of the work will be published later.

Thanks are due to the Chief Scientist, Ministry of Supply, for permission to publish this note.

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Nov. 1.

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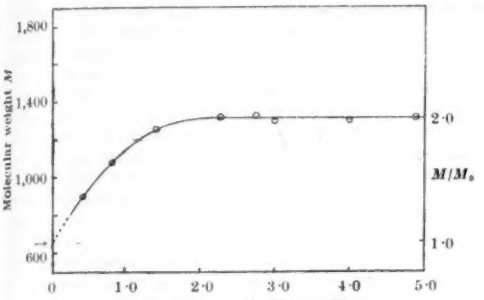
Monomeric Ferric Laurate in Toluene

METAL soaps occur as two distinct types of aggregate in non-polar solvents. The majority of mono- and di-valent soaps form large complexes held together by dipole association[1]. The trivalent aluminium compounds which are obtained as the basic mono- or di-soaps appear, on the other hand, to have a polymeric structure joined by metal–oxygen co-ordinate links[2] or by hydrogen bonds[3]. Solutions of the aluminium soaps exhibit characteristic visco-elastic phenomena and are believed to contain large fibrous aggregates.

Ferric laurate has been found to belong to neither of these two main types. This trivalent soap not only fails to exhibit the characteristic properties of the aluminium soaps, but is actually dispersed as single molecules in sufficiently dilute solutions in toluene. In stronger solutions, aggregation to the dimer occurs. Molecular weights of the soap determined by the Cottrell boiling-point method are shown in the accompanying graph.



Analysis gave the iron content of the soap as 8·75 per cent (calculated for $Fe(C_{12}H_{23}O_2)_3$, 8·54 per cent), indicating that the substance obtained was mainly the neutral tri-soap. In the absence of basic hydroxyl groups, the ferric tri-soap would not be expected to polymerize like the aluminium mono- and di-soaps. The explanation of its limited aggregation compared with other metal soaps appears to lie in the increased solubilizing power of its three hydrocarbon chains, as against two or one in the divalent and monovalent soaps. Steric hindrance to further aggregation may also be provided by the six hydrocarbon chains of the dimer.

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Nov. 22.

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Preparation of Chenodeoxycholic Acid from Cholic Acid

Kawai[1] converted cholic acid to chenodeoxycholic acid (3α : 7α-dihydroxycholanic acid, anthropodeoxycholic acid) by Kishner–Wolff reduction of the derived 3 : 7-dihydroxy-12-keto cholanic acid. Plattner and Heusser[2] similarly reduced methyl 3 : 7-diacetoxy-12-keto cholanate.

Recently, publication of improved methods of preparation of 3 : 7-dihydroxy-12-keto cholanic acid and its derivatives (see, for example, Fieser *et al.*[3]), together with the Huang-Minlon[4] method of reduction of ketones, have led us to re-investigate the production of chenodeoxycholic acid by this route on the preparative scale. We have needed this acid and its derivatives for comparison with material isolated from biles.

We have found, indeed, that Huang-Minlon reduction of 3 : 7-diacetoxy-12-keto cholanic acid proceeds smoothly on the 5-gm. scale to give chenodeoxycholic acid in approximately 70 per cent yield (compare Fieser and Rajogopalan[3]). The product can be 'crystallized' from ethyl acetate, and a (new) crystalline methyl ester (melting point 85–87°) was prepared from it by treatment at room temperature for 48 hr. with 1 per cent v/v sulphuric acid/methanol. This ester and the diacetyl ethyl ester (melting point 106–108°) were identical with material from chenodeoxycholic acid isolated from natural sources[5,6].

Full details of this work will be recorded elsewhere.

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[1] Kawai, S., *Hoppe-Seyl Z.*, **214**, 71 (1933).
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Preparation of Grey Tin

It is well known that most commercial forms of tin can be transformed to the grey (α) allotropic modification in an ordinary refrigerator by contact with particles of previously prepared grey tin. The difficulty, of course, is to get some grey tin to start with, and we know of many laboratories both in Britain and abroad in which attempts to make grey tin have been unsuccessful. We have worked on various aspects of grey tin from time to time, and, although we are not ready to give a connected account of this work, we wish to record a few of our observations in the hope that they may be helpful to other investigators.

The principle on which we make grey tin is to deform pure tin (99·99 per cent) by *cold work* and allow it to recrystallize at a temperature below the $\alpha \rightleftharpoons \beta$ transformation temperature (13·2° C.). The recrystallization temperature of tin is so low that deformation at room temperature is the equivalent of *hot work*. Tin may be conveniently cold worked by compression while surrounded by solid carbon dioxide. We generally compress under a pressure of 4 tons a cylinder 0·25 in. in diameter and 0·25 in. high. We then place the cold-worked tin in a glass tube and store it in solid carbon dioxide. It is almost completely transformed into grey tin powder within twelve to twenty-four hours, the rate of transformation increasing with the purity of the tin.

If a few grains of the grey tin thus produced are sprinkled on commercially pure white tin kept at a low temperature, the whole lot is transformed into grey tin, again at a rate which increases with the purity of the tin ; for example, we have transformed a kilogram of 'Chempur' tin into grey tin in about forty-eight hours.

While the foregoing is a fair statement of our general experience, we must report that one batch of 'spectroscopically pure' tin could not be trans-

formed by any means at our disposal, even when inoculated with grey tin powder. This was not the result of extreme purity, for (1) addition of various impurities to the sample failed to make it transformable, and (2) after the sample had been heated in a vacuum at 1,000° C. it could be transformed to grey tin with remarkable rapidity. We can only assume that this batch of tin was contaminated with a minute trace of some impurity which was removed by fractional distillation. A very small amount of sublimate was, in fact, collected; but no unusual material was identified in it. It seems remarkable and is perhaps of practical importance that so small an amount of matter can suppress the transformation entirely; but, whatever the explanation, the experiments have been repeated too often to leave room for doubt on the observations.

Arising out of this work, we have used the allotropic transformation to prepare white tin powder free from oxide. A bar of white tin, on which a few particles of grey tin are sprinkled, is sealed in an evacuated glass tube, which is then stored in solid carbon dioxide. After about forty-eight hours the tube is found to contain only grey tin powder, which can be converted to white tin powder by heating the tube in warm water. The particles of white tin formed in this way can have had no effective contact with gases and may be useful for certain investigations. We have used such material in special studies of the powder metallurgy of tin.

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Search for Beryllium-7 in Uranium Fission

Two types of ternary fission of uranium are known. In the first[1], two heavy fragments and a lighter particle of an average range 20–30 cm. in air are observed with a frequency of about 0·3 per cent of normal fission. The light particle is generally believed to be an α-particle; but Tsien *et al.* have suggested[2], from investigations using the photographic plate technique, that the mass may vary from 4 to 9. There is also some evidence[3] for the occurrence of beryllium-8. In the second type of ternary fission, two heavy particles and one light one with a range of about 8 mm. in air are observed[4,5], occurring to the extent of 1·3 per cent of binary fission. The light fragments are considerably more massive than α-particles and a mass of 13 ± 4 has been very tentatively suggested[5].

The isotope in the mass range 4–9 most suitable for a radiochemical investigation of the light particles is beryllium-7 with a half-life of 52·9 days[6]. Although it is on the neutron-deficient side of nuclear stability, unlike any of the known fission products, it was considered worth while to search for beryllium-7 in neutron-bombarded uranium.

A quantity of 50 gm. of uranium oxide which had been in the pile for three months was dissolved in nitric acid, about 100 mgm. of beryllium as nitrate added, the solution concentrated by evaporation and the majority of the uranyl nitrate removed by extraction with methyl *iso*butyl ketone. The residual aqueous solution was then treated with excess potassium hydroxide, which formed soluble potassium beryllate and insoluble potassium uranate. The solution was filtered and acidified with nitric acid, and beryllium hydroxide precipitated by addition of ammonia. Insoluble sulphide impurities were removed by three scavengings with a mixture of antimony and tellurium sulphides (approximately 15 mgm. of each). After boiling to remove hydrogen sulphide, about 15 mgm. of iron was added and precipitated as hydroxide in the cold with caustic soda to absorb impurities forming insoluble hydroxides. The precipitate was centrifuged, the mother liquors acidified and beryllium hydroxide precipitated by addition of ammonia. After washing the precipitate, it was dissolved in dilute acetic acid and the solution evaporated to near dryness, when 5 ml. of glacial acetic acid was added and the solution again evaporated. The basic beryllium acetate so formed[7] was extracted by several washes with chloroform. The organic solution was then washed with 0·5 *N* nitric acid solution and with water, the washings being rejected. The beryllium was recovered by evaporation of the chloroform and solution of the residue in hydrochloric acid. As a small amount of γ-activity was detected by a γ-scintillation counter, the formation and extraction of basic beryllium acetate was repeated, after which the beryllium was found to be inactive.

Assuming that 20 c./min. of beryllium-7 could be detected above the rather high background of the counter and that 11 per cent of the beryllium-7 disintegrations emit a 0·48-MeV. γ-ray[8], a limit to the fission yield was set from the known efficiency of the γ-counter, the chemical yield of beryllium, etc.; and the number of fissions was determined from the amount of barium-140 in suitable aliquots of the original solution. An upper limit of 2×10^{-5} per cent was obtained.

The experiment was repeated using 100 gm. of uranium metal irradiated for one year. A similar chemical procedure was employed, except that sodium uranate was used as a scavenging agent instead of ferric hydroxide, and the number of chloroform extractions of basic beryllium acetate was increased. No γ-activity above background could be detected in the beryllium fraction. In this case, using the same assumptions as before, the upper limit obtained for the fission yield was 10^{-6} per cent. Thus, assuming that ternary fission of the first type occurs to the extent of 0·003 of that of binary fission[1], less than one beryllium-7 nucleus is produced in 3×10^{4} ternary fissions of the first type and less than one in $1{\cdot}3 \times 10^{5}$ of the second type.

I wish to thank Mr. H. H. Willis for assistance in the chemical manipulations.

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Berkshire. Dec. 19.

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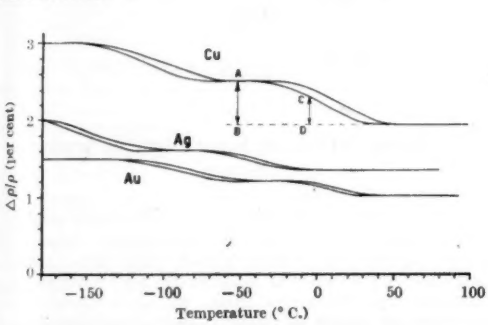
Recovery of the Resistivity of Metals after Cold-Working

In communications in *Nature*[1,2], measurements have been described on the recovery of the electrical properties of some metals after a deformation at liquid-air temperature. It appeared that this recovery occurred in two steps, if the temperature of the recovery-treatment was increased. These results were quantitatively rather rough, since the time necessary to bring the wires to the recovery-temperature could not be neglected.

More accurate measurements have now been carried out on gold, silver and copper wires. The accompanying graph shows the results. For each metal the recovery-times were 15 and 45 min. Vacant lattice sites can be accepted as the mechanism responsible for the increase of $\Delta\rho/\rho$. These vacant sites are produced during deformation and cause, together with dislocations, the change of the electrical properties. The change of the mechanical properties is assumed to be caused only by the pattern of dislocations.



Recovery of $\Delta\rho/\rho$ may be produced by diffusion of the vacant lattice sites, during the recovery-treatment, to regions where their influence on the resistivity is negligibly small. These regions may be dislocations or the grain- or mosaic-boundaries. With this assumption it can be shown that the average concentration of vacant lattice sites in a grain, $\bar{c}$, is, to a first approximation (for times not too small), given as a function of time by

$$\frac{\bar{c}}{c_0} = \frac{6}{\pi^2} \exp\left\{-\pi^2 \frac{Dt}{R^2}\right\}, \qquad (1)$$

where c_0 is the concentration for $t = 0$, D is the diffusion coefficient, and R is the radius of a sphere, which is assumed to be the shape of the grain or mosaic.

The relation between one time (t) and one temperature (T), which gives a certain value of $\bar{c}/c_0$, is

$$t = A \exp [Q/kT], \qquad (2)$$

as follows from (1); A is given by

$$A = \frac{R^2}{\pi^2 D_0} \ln\left\{\frac{6}{\pi^2} \cdot \frac{c_0}{\bar{c}}\right\}. \qquad (3)$$

Q is the activation energy for one jump of the vacant lattice site; D_0 is a constant in $D = D_0 \exp\{-Q/kT\}$.

These results can be applied to the experiments, since $c_0/\bar{c}$ corresponds with the relation AB/CD of the recovery-curves. It is thus assumed that the extra-resistivity is proportional to the average concentration of vacancies.

The following results were obtained:

(1) The value of Q can be calculated. This value appears to be a constant for one step of a certain recovery-curve. When the activation energy for the first step is called Q_1, and when Q_2 represents this energy for the second one, it was found that:

	Q_1 in eV.	Q_2 in eV.
Gold	0·29	0·69
Silver	0·18	0·65
Copper	0·20	0·88

This suggests that the mechanism of the second step is indeed the diffusion of single vacant lattice sites. It is possible that the first recovery step is produced by the diffusion of special aggregates of vacant lattice sites.

(2) When the value of A is plotted against $\ln\left\{\frac{c_0}{\bar{c}} \cdot \frac{6}{\pi^2}\right\}$, a straight line is obtained for gold and copper. This relation is predicted by the diffusion formula (3).

(3) With the experimental value of $C = R^2/\pi^2 D_0$ an estimate can be made of R. It was found that, for gold and copper, R is of the order of 1 μ.

I wish to thank the Delfts Hogeschool Fonds for financial support.

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Nov. 20.

[1] Molenaar, J., and Aarts, W. H., *Nature*, **166**, 690 (1950).
[2] Druyvesteyn, M. J., and Manintveld, J. A., *Nature*, **168**, 868 (1951).

Striations on Prism Faces of Quartz

Striations perpendicular to the Z-axis frequently occur on prism faces of quartz. They are often attributed to an 'oscillatory combination' of the prism and adjacent rhombohedron faces; but although this sometimes undoubtedly occurs (leading to tapered and 'candle' crystals), we believe that it is not the correct explanation of striations on well-developed crystals. A study of the surface topography of a number of natural crystals and a few synthetic crystals shows that the striations can be attributed to a difference in the rate of spreading of growth sheets parallel and perpendicular to the Z-axis.

The shape of the growth sheets on the prism faces of natural crystals is a parallelogram with two sides perpendicular to the Z-axis. The successive generation of growth sheets, from nuclei which frequently lie on the edge of the face or along a twin boundary intersecting the face, leads to the formation of low-angle growth pyramids. Fig. 1 is a photograph of these pyramids (Z-axis vertical). Two sides of the pyramids are much steeper and longer than the other sides, owing to the differential rate of growth parallel and perpendicular to the Z-axis. The steeper sides thus give rise to striations, although their gradient is at most 15°, that is, considerably smaller than the angle of 38° required by the oscillatory-combination theory. This explanation of the striations has also been given by A. F. Seager[1].

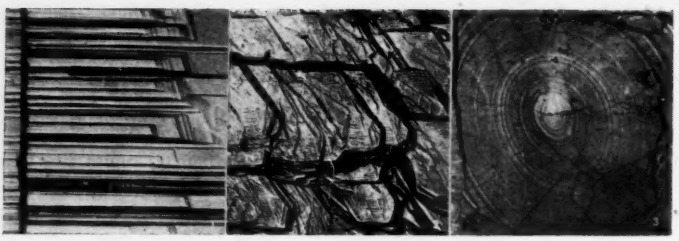

Fig. 2 shows growth pyramids giving rise to striations on the prism face of a synthetic crystal. The striations are not so well marked as on natural crystals, and they appear to run in two separate directions, as the differential growth-rate is here much less. Under other conditions of growth the synthetic crystals have no striations at all. It is then found that the growth sheets are circular (indicating growth velocity independent of direction) and give rise to conical growth pyramids, as shown on a rhombohedron face in Fig. 3.

Our thanks are due to Prof. S. Tolansky for his encouragement and interest in this work, part of which was carried out in his laboratory by one of us (B. T. M. W.).

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[1] Seager, A. F., Mineralogical Society, March 8, 1951.

Replacement of Plagioclase by Orthoclase in Deep-Sea Deposits

It is well known that authigenic orthoclase is present in fossil sediments of marine origin. In the course of my studies of fragments from gabbro and anorthosites in a core taken in 1948 by the Swedish Deep-Sea Expedition with the *Albatross* in the north-western Atlantic Ocean (lat. N. 29° 21′; long. W. 58° 59′; depth 5,450 m.), replacement of plagioclase (60–80 per cent anorthite) by authigenic orthoclase could be proved to have occurred. The replacement starts along fissures in the plagioclase. In fact, all stages in the transformation from fresh plagioclase to complete orthoclase were observed.

The authigenic orthoclase forms a framework of minute crystals of parallel optical orientation. This change is especially obvious in dolerite of typical ophitic texture, where the orthoclase is seen to form a framework around the lacunæ left by the dissolved plagioclase, which are dark in the accompanying photomicrograph.

This pseudomorphosis is limited to an external 'weathering' crust, 3–5 m. thick. Optical and X-ray analyses confirm that the mineral formed is a true orthoclase.

Otto Mellis

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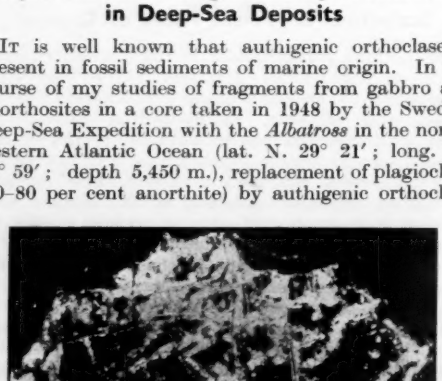
and
Oceanographic Institute,
Gothenburg.
March 1.



Authigenic orthoclase. × 20

Lower Miocene Invertebrates from Kenya

Recently, while continuing the search for additional Lower Miocene fossil fruits and seeds in the deposits on Rusinga and M'fwangano Islands in Lake Victoria, in order to obtain more data concerning the environmental conditions under which the Miocene ape *Proconsul* lived, we have found some remarkable fossils of insects and other soft-bodied invertebrates. This discovery is of unusual interest, not only because it is believed to be the first of its kind in Africa, but also because of the very unusual state of preservation of the material. Most fossil insect remains—other than those preserved in Oligocene ambers—have been found in a somewhat crushed and distorted condition in laminated rocks. In the present case the fossils, in spite of their soft-bodied nature during life, have retained their natural shape in a quite surprising manner. This is clearly shown in the accompanying illustrations of a lepidopterous larva (Fig. 1) and a pedipalpid (Fig. 2).

Some twenty specimens of invertebrates have so far been obtained, and it is anticipated that many

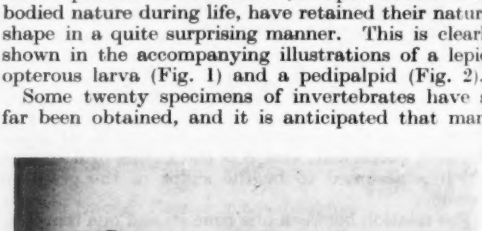


Fig. 1

Fig. 2

more will be found as the work of the British Kenya Miocene Expedition continues. The material at present includes (in addition to the specimens illustrated) representatives of Orthoptera, Coleoptera, Blattoidea and, perhaps the most surprising of all, what appears to be a fossil earthworm.

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Selection of the Genetic Basis for an Acquired Character

IN a letter under this title, Prof. C. H. Waddington[1] has described an interesting selection experiment with a *Drosophila* phenocopy of 'crossveinless'. He has demonstrated clearly that the phenocopic effect, at any rate in this case, depends on individual genetic constitution as well as on the shock treatment applied to the pupæ—a fact realized by Goldschmidt[2] for phenocopies in general, but not followed up as Prof. Waddington has done. Analogous results have been obtained in this laboratory using the mutant 'antennaless'[3]. Stocks homozygous for this gene show variable penetrance and expressivity on ordinary culture media. Selection and/or close inbreeding of individuals from such stocks can give rise to lines showing the whole range from almost zero to almost complete penetrance, representing the effects of the 'antennaless' gene when combined with different gene backgrounds[4]. The main difference between the two experiments seems to lie in the fact that in the crossveinless experiments the life-history includes a period of highly abnormal conditions, whereas in 'antennaless' the cycle is completed under normal culture conditions. The difference is, however, only one of degree, and the two situations would seem to be essentially parallel.

May I suggest, however, that it is perhaps unfortunate to extend the use of the term 'acquired character' beyond the idea of 'direct adaptation' referred to in Prof. Waddington's letter. Prof. Waddington's results, as he points out, are explicable in terms of selection of those genotypes which heat treatment reveals as being sensitive and capable of producing the crossveinless effect. I suggest that 'crossveinless' is no more an 'acquired character' than any other feature of an organism resulting from interaction of genotype and environment—and, in the long run it seems, we cannot logically say that any feature is not the product of such interaction. It appears, therefore, to confuse rather than to clarify the issue of the inheritance of acquired characters if the term is used in connexion with such experiments as these.

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Feb. 27.

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[2] Goldschmidt, "Physiological Genetics" (1938).
[3] Gordon and Sang, *Proc. Roy. Soc.*, B, **130** (1941).
[4] Begg and Packman (unpublished results).

IT is a very well-known fact, referred to in nearly all text-books of genetics, that the expression of many genes is influenced by the genetic background in which they find themselves, and that by selection and inbreeding in a uniform environment one can build up lines which differ in the frequency or intensity of manifestation of the mutant character. That Dr. Begg has shown that this is true of 'antennaless' does not seem to provide any very close parallel with the facts reported in my letter[1]. His thesis amounts to the suggestion that one is entitled to argue that, provided selection is effective in respect of any particular character, it must be effective in respect of all, since all characters are to some extent dependent on the genotype. This somewhat abstract truism was, of course, one of the lines of thought which led me to perform the experiments recorded in my letter, and prevents one being surprised at the demonstration that it is possible to select successfully for the capacity to respond to an abnormal environment; but an *a priori* justification is not quite as informative as the experiment itself, since it cannot tell one how effective the selection will be. Further, neither Dr. Begg's argument nor his experiment leads to the conclusion that, after several generations of selection in the abnormal environment, the character will continue to be developed by animals cultured under normal conditions; and this was the essential point.

The expression 'an acquired character' can, as many people have pointed out, be so interpreted that it becomes tautologous. If one approaches it on a philosophical plane, disregarding the conventional technical meaning which has grown up around it, one can consider all characters as 'acquired', since all depend on the environment as well as on the genotype. But conventionally the phrase is taken to refer to a character not usual for the species, formed by an organism which develops in an environment other than that in which the species usually lives or has lived in the immediate past. The question then arises whether such characters are inherited, and if so, how.

The point was perfectly clearly stated by Goodrich as long ago as 1924[2]: "Every character is the product both of factors of inheritance and of environment. . . . Characters are due to responses, and have to be made anew at every generation. Only those characters reappear regularly in successive generations which

depend for their development on stimuli always present in the normal environment. Others, depending on a new or occasional stimulus, do not appear in the next generation unless the stimulus is present". The point of my communication was to show that in certain circumstances the last sentence of this quotation is not necessarily true.

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Validity of the *Neurospora* Back-Mutation Test

In a recent communication, Grigg[1] has discussed the possibility of an interconidial inhibition in *Neurospora*, conidia of biochemically deficient mutant strains inhibiting the growth of wild-type conidia. It was demonstrated that large numbers of mutant conidia inhibit the growth of wild-type conidia by mixing varying concentrations of mutant conidia with small aliquots of wild-type conidia and subsequent plating on 'minimal' medium. The mutant conidia will not grow on minimal medium, although they germinate; the wild-type conidia should grow normally. Using, for example, an adenineless mutant (*W*40) of a macroconidial strain, 100 per cent inhibition of wild-type conidia was observed in the presence of 4×10^7 adenineless conidia per plate, whereas no inhibition was found in the presence of 2×10^7 adenineless conidia.

On the basis of this observation, the validity of the *Neurospora* back-mutation test for investigating the mutagenic effect of chemical and physical agents is questioned. This test has been used in our laboratory, and in a number of other laboratories, in recent years for studying the mutagenic effect of various treatments (for references see Grigg[1]). The increased number of wild-type colonies which is observed after treatment with mutagens is explained by Grigg as a "dilution effect": the treatment kills so many spores that the interconidial inhibition is overcome.

Grigg, however, has overlooked the fact that the experimental conditions under which the interconidial inhibition is observed are different from those under which the back-mutation test is carried out. In Grigg's experiments the conidia are plated on a minimal medium containing 0·2 per cent sucrose and 0·4 per cent sorbose. The latter is not used as a source of carbon by the organism. The medium used in the back-mutation experiments contains 1·5-2·0 per cent glucose or glycerol, and no sorbose.

We have repeated the reconstruction experiments by mixing varying concentrations of adenineless conidia (*W*40) with small aliquots of wild-type spores originating from a reverted *W*40 strain. The conidia were plated both on a sorbose medium similar to Grigg's and on our standard medium with 2 per cent glucose. The results of one experiment are shown in the accompanying table. The experiments have consistently shown that the inhibition effect observed by Grigg is observed on the sorbose medium only, whereas no inhibition is found on the glucose medium. We believe this discrepancy is due to the fact that the sorbose medium is a starvation medium,

	Number of colonies					Strain and medium
ad. −	0	$1·1 \times 10^7$	$2·2 \times 10^7$	$4·4 \times 10^7$	$8·8 \times 10^7$	
ad.+ colonies	53	55	40	0	0	*W*40, sorbose
	41	41	47	42	43	*W*40, glucose

containing a limiting amount of glucose. When the mutant spores germinate, they may use up all the glucose, and no more carbon is available for the further growth of the wild-type conidia. This limiting factor does not occur in the standard glucose medium. Since the latter medium is used exclusively in the back-mutation experiments, we conclude that Grigg's observation, although interesting in itself, has no relation to the back-mutation experiments, and that his criticism of this test is invalid.

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Fertilizing Capacity of Bull Spermatozoa after Freezing at −79° C.

In 1949, Polge, Smith and Parkes[1] reported that glycerol had remarkable properties in protecting spermatozoa against the effects of low temperatures. The most promising results were obtained with fowl spermatozoa, which, frozen rapidly to − 79° C. (solid carbon dioxide) or − 192° C. (liquid air) in 15 per cent glycerol, resumed full motility on thawing. Later, it was found that fowl spermatozoa so treated were capable of effecting fertilization of the egg[2]. Mammalian spermatozoa have proved more refractory. The spermatozoa of several species—rabbit, guinea-pig and horse[3], bull and goat[4]—can be revived to show some degree of motility after freezing at − 79° C. in 15 per cent glycerol. However, repeated experiments with the rabbit, the only laboratory animal suitable for artificial insemination, have, as yet, resulted in the fertilization of only one egg by spermatozoa so treated[3], and early tests with frozen bull spermatozoa carried out at Shinfield were unpromising, though one calf was reported to have been produced[5].

The object of the present communication is to report the results of two series of experiments carried out on bull spermatozoa at Cambridge. In the autumn of 1950, semen samples were divided into three parts and diluted 1 : 1 at + 30° C. with (1) Wellcome citrate buffer containing 50 per cent egg yolk; (2) and (3) a similar yolk-buffer mixture containing 30 per cent glycerol. Samples (1) and (2) were then stored for 24 hr. in the refrigerator at + 5° C. Sample (3), shortly after dilution, was cooled slowly overnight from + 30° C. to − 79° C., and thawed at + 40° C. the following day. Rates of revival from the frozen semen were poor, and were not much improved by adding the glycerol slowly or by varying the rate of freezing. About twenty cows were used for testing semen of each of the three categories, and those not returning to œstrus within

six weeks after insemination were checked for pregnancy by rectal palpation. The result was that the normal, glycerolized and glycerolized frozen semen specimens gave pregnancy-rates of 50, 76 and 0 per cent, respectively.

This work proved conclusively that glycerol, in a concentration of 15 per cent, did not damage the fertilizing capacity of the spermatozoa. By contrast, it suggested strongly, though not quite with statistical significance on the small numbers, that the addition of glycerol improved the effectiveness of normal semen. The negative results obtained with frozen semen were disappointing and new methods were sought when work was resumed in the autumn of 1951. First, in view of the indication that frozen cells deteriorate rapidly on storage *in vitro* after thawing[5], ten cows were inseminated with semen, frozen as before, but thawed on the farm immediately before insemination. Again no pregnancies were obtained.

Work was then continued on the conditions of freezing, and it was found that the rate of revival could be substantially increased if the semen was equilibrated for several hours with the glycerol diluent before freezing. The following technique was therefore adopted. Semen, immediately after collection, was diluted with an equal volume of yolk-citrate buffer at + 28° C. and allowed to cool to + 5° C. during 4 hr. in the refrigerator. The preparation was then diluted with an equal volume of citrate buffer containing 20 per cent glycerol at + 5° C. and allowed to stand overnight at + 5° C. The semen was then cooled slowly for 45 min. from + 5° to − 79° C. and stored in sealed ampoules immersed in solid carbon dioxide and alcohol. Samples were kept frozen for periods varying from 2 hr. to eight days, and were thawed immediately before insemination. Thirty-eight cows were inseminated with semen so treated. Six of these returned on œstrus within three weeks; the remaining thirty-two were checked for pregnancy six weeks after insemination. Thirty were found definitely to be in calf, a pregnancy-rate of 79 per cent. As the frozen semen was diluted only 3 : 1, far more spermatozoa were used for each insemination than would normally be the case. It is not possible, therefore, at this stage to assess the proportion of cells which retain functional effectiveness after freezing and thawing by the method described; but it is evident, in a general way, that exposure of bull spermatozoa to low temperatures is compatible with the retention of fertilizing power. The fact that a pregnancy-rate was obtained which would be considered exceptionally good for normal semen, in any concentration, is probably to be ascribed, in the light of the 1950 experiment, to the use of glycerol rather than to the effects of freezing.

On the theoretical side, this work has significant implications about the permeability of cells and their reactions to low temperatures. On the practical side, it opens up the possibility of storing bull semen at temperatures far below those used at present. This new technique can scarcely fail to increase greatly the length of time for which bull semen can be preserved in a potentially functional condition at insemination centres; but much more work may be necessary to determine conditions compatible with very prolonged survival of the spermatozoa.

The experiments described above form part of the programme of work on the effects of low temperature on living cells being carried out by the Division of Experimental Biology, National Institute for Medical Research, and we wish to acknowledge the help and encouragement of Dr. A. S. Parkes and Dr. A. U. Smith. We are also much indebted to the committee of the Cambridge Cattle Breeders' Society for co-operation, and to Mr. D. L. Milne, who carried out the experimental inseminations. The expenses of the work were borne jointly by the Medical Research Council and the Agricultural Research Council.

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Respiratory Quotient of the Isolated Bovine Mammary Gland

Making use of the arterio-venous method for the determination of the respiratory quotient of the lactating udder of the ruminant, different authors[1–5] found mean values greater than unity. These results were interpreted[1,2] as evidence of the formation of fat from carbohydrate. More recently, Folley and French[6] carefully reinvestigated the problem and observed in the lactating cow a mean value of 1·25 ± 0·12.

As these high values were not obtained by studies on perfused glands[3,5], Folley and French[6] determined the respiratory quotient of mammary gland slices from the lactating rat, and found in the presence of glucose a mean value of 1·54 ± 0·03. The same authors observed[7], however, that for slices from the goat's udder the respiratory quotient is well below unity when glucose is given as the substrate. When glucose was replaced by acetate, the respiratory quotient rose to values definitely greater than unity. According to these authors[7], the mammary tissue of the ruminant appears to be adapted for metabolizing acetate; the acetate is oxidized, and it is also utilized for fat synthesis. Experiments with labelled carbon confirmed these facts[8].

We have now measured, taking all possible precautions, the respiratory quotient of the perfused isolated cow's udder. The udder was perfused by cow's blood according to the method we described earlier. The two halves of the udder were perfused separately: one half was supplied with glucose; in the perfusion liquid of the second, half the quantity of glucose was replaced by acetate. Samples of the perfusion liquid were taken from time to time and, after having made sure that no hæmo-concentration occurred, oxygen and carbon dioxide were determined by the method of Van Slyke. In all our experiments the venous output was identical in both halves of the udder and remained practically constant during the perfusion experiment.

In the accompanying table we give the results of a typical experiment. It will be seen that, apart from the first determination, values well above unity are obtained when acetate is present in the perfusion

RESPIRATORY QUOTIENT OF COW'S UDDER

To the perfusion liquid of the left half, glucose (15 gm./hr.) was added continuously. Venous outflow = 500 ml./min.
To the perfusion liquid of the right half, glucose (7·5 gm./hr.) and sodium acetate (7·5 gm./hr.) were added continuously. Venous outflow = 500 ml./min.

Time	Respiratory quotient	Oxygen utilization (vol. per cent)	Carbon dioxide content, arterial blood (vol. per cent)	Carbon dioxide content, venous blood (vol. per cent)	Oxygen content, arterial blood (vol. per cent)
Left half of udder					
30 min.	0·78	4·8	37·98	41·73	19·70
45 min.	0·91	4·7	37·28	41·57	19·45
60 min.	1·09	4·3	37·72	42·39	19·05
75 min.	1·06	3·7	38·20	42·11	18·50
90 min.	0·87	3·1	38·79	41·46	18·20
105 min.	1·06	3·4	38·38	42·00	18·20
Right half of udder					
30 min.	0·98	8·55	37·94	46·32	20·05
45 min.	1·11	8·50	38·41	47·88	19·95
60 min.	1·17	8·00	40·77	50·16	19·80
75 min.	1·22	6·77	46·03	54·29	19·02
90 min.	1·13	6·05	49·88	56·71	18·35
105 min.	1·45	5·15	52·17	59·68	18·45

liquid. The low value for the first determination, which was also observed in other experiments, is, in our opinion, due to a certain degree of damage caused by the interruption of the circulation necessary to excise the gland and to prepare the perfusion experiment. With glucose alone we never obtained mean values greater than unity.

It will also be observed that oxygen consumption is much higher in the acetate half. It declines as a function of time, probably as a result of damage by the perfusion. Whereas carbon dioxide remains constant in the glucose half, it rises steadily and significantly in the acetate half, as a result of acetate utilization.

In all our experiments the oxygen content of the arterial blood varied between 17 and 21 per cent. These figures agree well with the degree of oxygenation of the arterial blood in the living animal.

Our results confirm those obtained with living animals[1-6], and with slices. They stress the important observation of Folley and French[7] on the utilization of acetate by the ruminant udder, and are in accordance with the results of Petersen and Shaw[3,5], who, with their perfusion technique, found values of the respiratory quotient lower than one in the absence of acetate in the perfusion liquid. They also prove that our perfusion method may be very useful in studying milk synthesis.

Full details of these experiments will be published elsewhere.

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Origin of the Resistance of Leukæmic Cells to Folic Acid Antagonists

LEUKÆMIC cells of the mouse develop resistance to various analogues of folic acid[1,2]. The variant cells in certain cases depend upon the 4-amino-substituted folic antagonists for optimal growth[3]. The changes are found to be stable, irreversible and heritable[4], and occur generally among many transplantable leukæmias[5].

To explain the occurrence of the numerous variants among bacteria that grow in environments unfavourable to the parent strain (antibiotics, etc.) it may be assumed (1) that the unfavourable environment induces the change in some of the organisms, or (2) that the variant types arise as mutants during normal growth, the unfavourable environment being merely selective in their isolation. Luria and Delbrück[6] utilized the 'fluctuation test' to distinguish these two possibilities. Using this experimental method on widely separated strains of bacteria showing resistance to many different substances, numerous investigators have concluded that the variant type arose by spontaneous mutation[7]. More direct evidence that spontaneous changes occur in the sensitivity of *E. coli* to phage[7] and to streptomycin[8] has been reported.

The fluctuation test was adapted to a study of the origin of resistance of leukæmic cells to folic acid antagonists. At the 105th serial transfer of the sensitive line of leukæmia *L* 1210 in *dba* strain mice, a small inoculum (150 cells) was transplanted subcutaneously into a *dba* mouse. A large lymphomatous growth developed, and from this fifteen independent transfer sublines were started. The independent sublines (1–15) were continued through six successive transfers in untreated mice. A standard dose of leukæmic cells (8×10^5) in Locke's solution was used as inoculum in each case, and all transfers of lymphomatous tissue were done at nine days[2,3].

At the seventh serial transfer of the fifteen *independent* sublines, standard doses of leukæmic cells were then inoculated subcutaneously into ten mice for each subline, and a total of four injections of *A*-methopterin (2·5 mgm./kgm. daily) given every other day, beginning twenty-four hours post-inoculation. Subline 13 served as a control test. In this control series, ten groups of ten mice each were inoculated with the standard dose of leukæmic cells and given *A*-methopterin. This subline represents repeat tests of this population of leukæmic cells. Thus, two different sets of tests were made: one in which leukæmic cells were derived from fifteen *independent* sublines, and one in which leukæmic cells were from the same (*single*) subline.

To determine whether resistant cells existed in the lymphomatous tissue used as the source, ten *dba* mice were inoculated with a standard dose of leukæmic cells from the 106th transfer that developed from the original inoculum of 150 cells. These mice were then given 2·5 mgm./kgm. of *A*-methopterin every other day for four days (10 mgm./kgm. total), the same dose used in all tests. No growths were obtained in these mice at nine days. It follows that any resistant cells found in the independent sublines arose during the subsequent seven serial transfers.

At nine days post-inoculation, animals were sacrificed and the lymphomatous tumour tissues weighed (see table). Variation in the independent subline series, started from a common source, was highly significant. Two of the sublines contained no resistant

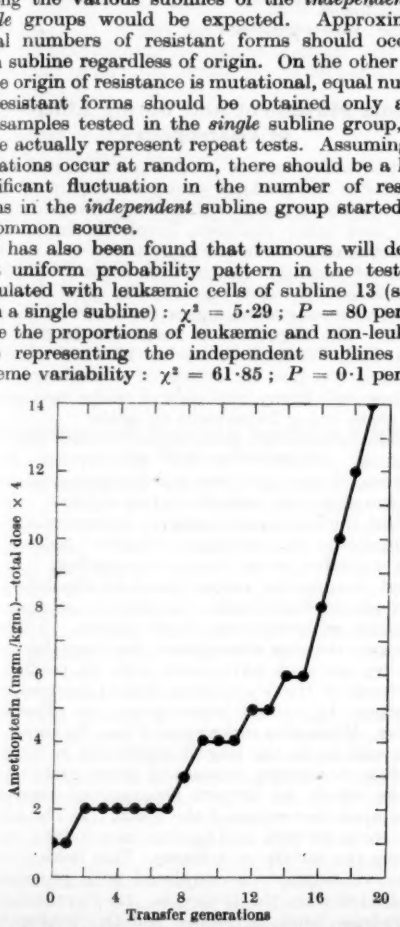
VARIANCE ANALYSIS OF THE MEAN WEIGHTS OF LYMPHOMATOUS TISSUE IN INDEPENDENT AND SINGLE SUBLINES OF LEUKÆMIA L 1210

Independent sublines				Single subline			
Subline No.	No. +	Growths −	Mean wt. (mgm.) lymphoma tissue	Subline No.	No. +	Growths −	Mean wt. (mgm.) lymphoma tissue
1	9	1	181·9	13 *a*	6	4	22·0
2	3	7	8·5	13 *b*	5	5	67·7
3	3	7	8·0	13 *c*	6	4	51·0
4	10	—	99·3	13 *d*	8	2	54·5
5	6	4	69·2	13 *e*	6	4	58·0
6	8	2	30·3	13 *f*	6	4	36·2
7	7	3	31·0	13 *g*	6	3	41·4
8	—	10	0·0	13 *h*	8	2	47·4
9	3	7	7·5	13 *i*	8	2	27·0
10	9	1	88·3	13 *j*	8	2	42·4
11	9	1	199·0				
12	—	10	0·0				
14	4	5	74·8				
15	7	3	68·1				
	Mean = 61·85				Mean = 44·8		
	Variance = 4,144·84				Variance = 195·9		

forms. The variance ratio between the independent and single subline series was twenty, with $P < 0{\cdot}001$.

If resistant variants arise by interaction of leukæmic cell and antagonist, no significant differences among the various sublines of the *independent* and *single* groups would be expected. Approximately equal numbers of resistant forms should occur in each subline regardless of origin. On the other hand, if the origin of resistance is mutational, equal numbers of resistant forms should be obtained only among the samples tested in the *single* subline group, since these actually represent repeat tests. Assuming that mutations occur at random, there should be a highly significant fluctuation in the number of resistant forms in the *independent* subline group started from a common source.

It has also been found that tumours will develop in a uniform probability pattern in the test mice inoculated with leukæmic cells of subline 13 (sample from a single subline): $\chi^2 = 5{\cdot}29$; $P = 80$ per cent, while the proportions of leukæmic and non-leukæmic mice representing the independent sublines show extreme variability: $\chi^2 = 61{\cdot}85$; $P = 0{\cdot}1$ per cent.



Step-wise increase in resistance of leukæmia *L* 1210 to folic antagonist. Transfer generation 110 used as source

Discrete stepwise increases in resistance of leukæmic cells of leukæmia *L* 1210, resembling the pattern of penicillin resistance in bacteria[9], have been observed to occur during serial transfers in mice receiving graduated dosage levels of folic antagonist from 1·0 to 14·0 mgm./kgm. (total: 4·0–56·0 mgm./kgm.) (see graph).

The results—(1) extreme variation in the numbers of resistant leukæmic cells among samples of *independent* sublines started from a common source; (2) increases in resistance in a stepwise fashion during serial transfer; and (3) stability of resistance in the absence of antagonist—would seem to favour the assumption that mutation and selection constitute the mechanism by which resistant leukæmic cells develop.

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Wheat Midge

DURING last year an unusually severe attack by wheat midge larvæ, *Contarina tritici* (Kirby) and *Sitodiplosis mosellana* (Gehin), caused serious damage to wheat crops in some districts in Ireland, particularly in the southern counties. *C. tritici* was very much the predominant species, and so the damage caused was due mainly to sterility of the florets. In exceptional cases the estimated damage was as high as 70 per cent. The damage varied considerably in different counties; but there was also great variation between different districts in the same county and, indeed, between different fields on the same farm. The loss of crop due to midge attack was estimated at 30 per cent in County Wexford, 20 per cent in County Cork, 15 per cent in County Waterford and 10 per cent in County Limerick.

As there appears to be little information available on the control of wheat midge, it may perhaps be worth recording the following observations. In late June it was observed that the midge population in the nursery plots of wheat at the Cereal Station, Ballinacurra, Co. Cork, was very high. Disturbance of the foliage caused a cloud of midges to arise from the crop. It was anticipated that damage would be severe, and it was decided to spray the plots with DDT. The spray, made up by mixing a proprietary DDT preparation with water in the proportion of 1 : 49, was applied with a knapsack sprayer at 50 gallons per acre on June 28. At that time a few of the plots had eared out, but the greater number were at the immediate pre-emergence stage.

The spray gave a very good kill. Dead midges could be observed in great numbers on the foliage, and on the succeeding days it was very difficult to find any live midges in the plots. Extremely good control was obtained in all plots which had not eared out before the spray was applied. In the few plots

which were in ear at the time of spraying, extensive damage occurred, and it was estimated that 35 per cent of the florets had been destroyed. In these plots the eggs of the midge could be readily found inside the glume at the time of spraying, and it was obvious that oviposition had taken place to a very considerable extent.

As is well known, egg-laying by *C. tritici* commences as soon as the ear emerges and, in fact, it was observed in some plots that eggs were deposited in the glumes through the slit in the leaf sheath before ear emergence had properly occurred at all. It would appear, therefore, that in order to achieve satisfactory control with DDT, the objective should be to kill the adult midges before they have an opportunity to lay their eggs.

This experience with small plots indicates that good control of wheat midge can be obtained by the application of a suitable insecticide at the appropriate time; but whether spraying on a field-scale would be a feasible proposition is another matter. There are many obvious difficulties attached to spraying on a large scale, and it may be that the use of smoke generators would offer a better solution to the problem. These have been used to some extent in Sweden[1], and it is proposed to investigate their use in Ireland next season. Having regard to the importance of biological control in the case of wheat midge, the use of DDT is objectionable, since it will also destroy the natural enemies of the pest. Nevertheless, the use of an insecticide would appear to be justified in a year when the midge population is at its peak and when weather conditions are sufficiently favourable to ensure a heavy infestation, with consequent serious damage to the wheat crop. The adult midge population in the crop shortly before ear emergence would appear to be a good criterion on which to make the decision as to whether an insecticide should be applied or not. In this connexion the development of a reliable technique for the estimation of the expected midge population and investigation into the relationship between population and reduction in yield would seem to be worth pursuing.

So far as we are aware, there is little information available regarding varietal reaction to wheat midge. Wahlin[2] suggests that variety is of importance in relation to the extent of wheat midge infestation, but he attributes any differential varietal behaviour to differences in time of ear emergence. He found no significant difference in the degree of infestation as between several varieties. Advantage was taken of a spring wheat variety trial located at Johnstown Castle, County Wexford, which was heavily attacked by wheat midge, to make a preliminary investigation of any differential varietal resistance. The trial comprised four varieties, all belonging to *T. vulgare*, with ninefold replication. A sample of ears selected at random was taken from each of the ten plots of each variety and the number of fertile and sterile florets determined for each ear. In making the counts, the normally occurring sterile florets at the base and tip of the ear were excluded. A total of 250 ears was examined with the following result:

Percentage of Sterile Florets

Atle	Karn II	Progress (*a*)	Progress (*b*)	Ella
34·4	48·4	59·2	53·4	63·8

Significant difference = 6·98 (P = 0·01)
" " = 9·20 (P = 0·001)

Progress (*a*) was grown from Swedish seed and Progress (*b*) from Irish seed.

The dates of ear emergence were: Karn II, June 29; Ella, July 1; Progress, July 6; Atle, July 6.

These results show that Atle suffered much less damage than any of the other varieties. The lower figure for Atle cannot have been due to an escape factor, since this variety and Progress came into ear simultaneously. Confirmation of the relative resistance of Atle to wheat midge was obtained on many farms throughout the affected areas at threshing time, the general experience being that the variety Atle yielded much better than Progress. The differential reaction of Atle and Progress, which are the two main varieties grown in Ireland, proved to be a matter of considerable economic importance to farmers in the past season. No information is available as to the nature of the resistance of Atle; but it is hoped to investigate further this aspect of the problem.

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[1] Wahlin, B., Växtskyddsnotiser No. 2 (1949) från Statens Växtskyddsanstalt, Sweden.
[2] Wahlin, B., Växtskyddsnotiser No. 5 (1945).

Radula Movement and Boring Efficiency of Gastropods

Radula movement in prosobranchiate gastropods is a factor in the predator-prey relationship of many species of gastropods (Naticidæ, Muricidæ and others) and other molluscs, involving considerable masses of animal life in the sea.

Jensen[1,2] recently stated that certain Naticidæ, and inferentially other gastropods, drill holes by mechanical means alone and without aid of enzymes or acids. His argument is based on the fact that *Natica* drills victims in about 27 hr., which he believed is too fast for action of a weak acid, and that they drill horny egg-cases of skates and gastropods, these being impervious to acids.

Certain additional information shows the high mechanical efficiency of the prosobranch boring apparatus. Years ago there was disagreement among zoologists about the way the radula worked. Cuvier[3] described the radula as a passive instrument moved secondarily by its cartilages. Huxley[4] believed the radula operated as a band-over-a-pulley. Patrick Geddes[5] thought the radula was held absolutely still on the odontophore frame. Apparently all argument was based on anatomical observations. Adherents of the two theories throughout the years have been listed by me[6]; I have been able to confirm the correctness of Huxley's views and to analyse radula movement by direct observation on *Fasciolaria gigantea*, *Melongena corona* and *Thais floridana*. The first is said to be the largest gastropod in the world.

Radula movement consists of three parts or components which all impart separate or cumulative speeds upon movement of the teeth. (1) The odontophore rocks forward and upward in a licking motion, carrying the radula on a frame. This licking motion of the odontophore is correlated with peristalsis of food particles up the proboscis. (2) Simultaneously, the radula band is pulled rapidly backwards in a groove like a belt over a wheel, with the ex-

ception that the wheel does not turn. (3) As the radula band moves over the odontophore, the teeth are suddenly spread and move in the locus of the circumference of a small wheel. This imparts a third speed to the other two, and in effect the radula of prosobranch gastropods operates as if it were a small drum covered with spikes moving back and forth regularly. Obviously, it cannot act continuously as a revolving drum, but must do so discontinuously.

Later, Carriker[7] verified observations concerning band-over-pulley movement of the radula by examination of *Urosalpinx cinerea* with low-power microscopes. His work was independent, and apparently he was not acquainted with my paper.

Thus, the efficiency of mechanical movement of the radula seems to be well established, not only by such facts as those given by Jensen but also by direct observation. However, this mechanical efficiency does not rule out the possibility of supplementary aids by acid or enzyme. The time of action of a dilute strongly dissociated acid may be rapid, unless it is extremely dilute, and weakly dissociated organic acids would be of little benefit in any event. A gastropod which was mechanically efficient in boring, but which also utilized supplementary secretions, could feasibly bore chitin or shell without use of secretions. It would appear, then, that the only way of disproving the assertions of Ankel[8] and others concerning secretions would be to repeat their experiments.

GORDON GUNTER

Institute of Marine Science,
The University of Texas.
Nov. 1.

[1] Jensen, A. S., *Nature*, **167**, 901 (1951).
[2] Jensen, A. S., *Vidensk Medd. fra Dansk naturh. Foren.*, **113**, 251 (1951).
[3] Cuvier, G., "Memoire pour servir à l'histoire et à l'anatomie des mollusques" (Paris, 1817).
[4] Huxley, T. H., *Phil. Trans. Roy. Soc.*, **143**, 29 (1853).
[5] Geddes, P., *Trans. Zool. Soc. London*, **10**, 485 (1879).
[6] Gunter, G., *J. Acad. Sci., Wash.*, **26**, 361 (1936).
[7] Carriker, M. R., *J. Morphol.*, **73**, 441 (1943).
[8] Ankel, W. E., *Zool. Anz.*, **11**, Supp. 223 (1938).

Occurrence of *Puccinia polysora* Underw. in West Africa

A RUST disease of maize has been known in West Africa for many years. Collections prior to 1949 were all of *Puccinia sorghi* Schw., both uredo- and teleuto-spores. The damage done to the crop was generally very slight; but in 1949 one of us (F. C. D.) found an epidemic of maize rust in Sierra Leone. In 1950 epidemics were reported from Liberia, Ivory Coast, Gold Coast, Dahomey and Nigeria, causing serious damage in all these countries, estimated up to 50 per cent of the crop in some areas, which resulted in local food shortages.

The 1949 collections of maize rust from Sierra Leone were at first thought to be *P. sorghi*, but the virulence of the epidemic and the somewhat larger size of uredospores aroused suspicions. Dr. G. R. Bisby, of the Commonwealth Mycological Institute, undertook the critical examination of the material and also of collections made in 1951 from the Gold Coast and Nigeria (J. M. W.). He concluded that all these collections belonged to *P. polysora* Underw., and Dr. G. B. Cummins, of the United States, confirmed this. Hitherto, only the uredo stage of this species has been found in West Africa.

A re-examination of all African specimens of maize rust in the herbarium of the Commonwealth Mycological Institute made in 1944, following Cummins's[1] report in 1941 of *P. polysora* Underw. on maize from America, showed all the specimens to be *P. sorghi* Schw. The Sierra Leone collections of 1949 thus appear to be the first record of *P. polysora* Underw. outside the western hemisphere.

The Gold Coast record of *P. polysora* Underw. in 1951 is of particular interest, as a previous collection of maize rust made by S. J. Hughes[2] in that country so recently as 1949 proved to be *P. sorghi* Schw., with telia. Had *P. polysora* Underw. been present at that date with the intensity it appeared in the 1950–51 epidemics, it could scarcely have been overlooked. This suggests that the fungus arrived in the Gold Coast not earlier than 1949.

How *P. polysora* Underw. reached West Africa is unknown; but it may have been introduced with importations of maize from the Americas, made from time to time, both as seed and in bulk for food. The literature on *P. polysora* Underw., first described in 1897, is scanty compared with that on *P. sorghi* Schw., which has been extensively studied in America. Dr. Cummins, however, has since found that a number of collections labelled "*P. sorghi*" dating back to 1879 prove to be *P. polysora* Underw.

D. RHIND
West African Inter-Territorial Secretariat,
Accra, Gold Coast.

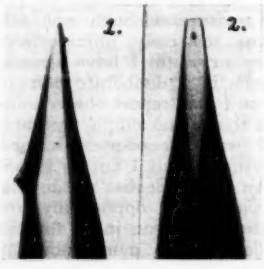
J. M. WATERSTON
Department of Agriculture,
Ibadan, Nigeria.

F. C. DEIGHTON
Department of Agriculture,
Njala, Sierra Leone.
Dec. 18.

[1] Cummins, G. B., *Phytopath.*, **31**, 856 (1941).
[2] Bisby, G. R. (in correspondence).

A Micro-Injection Needle

MICRO-PIPETTES are blunt by their very physical nature. A micro-injection needle would be a more appropriate instrument for penetrating and injecting small biological specimens. Such a needle is shown in the photographs (1, side view; 2, viewed from above). It has a chisel-like extremity. The microscopical glass-forging method[1,2] by which it is prepared permits orifices of all sizes to be made, with a lower limit of a few microns. It is made by drawing out a capillary tube in which a lateral hole has been blown. When the extremely narrowed tube breaks, half the elongated hole is severed. The remaining half has become terminal.



It is hoped that this needle will penetrate where micro-pipettes do not.

Jan Worst

Mozartstraat 1,
Arnhem.
Feb. 1.

[1] Schouten, S. L., *Z. wissensch. Mikr. und Mikr. Techn.*, **51**, 421 (1934).
[2] de Fonbrune, P., "Technique de Micromanipulation" (Masson et Cie., Paris, 1950).

"Racial Studies"

I prefer not to answer ideological attacks on my writings, such as Cedric Dover's review[1] of my pamphlet, "The Roots of Prejudice"[1], published by Unesco; but since Unesco is attacked as well as myself, I think I should reply. Unesco is not an "advertising agency for dubious scholarship"—as Mr. Dover calls it—but an international organization for the promotion of mutual understanding and respect among men, and it chooses to use pamphlets like mine as one means to its goal. My pamphlet was not an expression of my own point of view, but a summary (unfortunately brief and popularized) of all the present-day theories advanced to explain prejudice, including the one favoured by Mr. Dover—the economic exploitation theory. I stressed the fact that, despite the excellent researches that had been carried on, we have not yet reached a scientifically conclusive answer to the causes of prejudice.

Let me take up briefly Dover's accessory points. (1) It is quite true that I did not go back to "Antarah, Ibn Khaldun, and Vico" as sources; but I could scarcely be expected to do so in a popularized pamphlet on contemporary research and theories. (2) Mr. Dover's inability to discover any persons or cultures that have or had no prejudice is surprising even to one familiar with his ideology. (3) I will reiterate that sometimes slavery and imperialism do not rely on racism, and merely cite Roman imperialism and slavery in the Western world between 1500 and 1800 to illustrate my contention. Granting that slavery and imperialism are vicious, we need not distort history to contend that they are always encumbered with all the evils imaginable. (4) I have already cited evidence that imperialism and slavery can retard a nation economically as well as psychologically, and I have never stated that this was always the case. (5) I do not think it necessary to answer Mr. Dover's attack on psychiatry in general, except to say that whatever its defects, it is struggling honestly to be an objective science. (6) If Mr. Dover wishes to go into such word-twisting as "all the great religious conflicts have been essentially political" and political motives are a "cliché concealing a complex of economic motivations" that is his privilege; but I have followed general usage in distinguishing between religious (or ideological), economic and political motivation. (7) The historians on prejudice have shown that racism, although not other forms of prejudice, was relatively unimportant before the nineteenth century. (8) I have already stated that some ignorance is a deliberate camouflage for exploitation, but I still report the conclusion of many psychologists that some simple ignorance also exists.

Finally, I wish to respond vigorously to Mr. Dover's indictment that I am an idealist, and, even worse, an "American idealist". I do not deny it, and am indeed pleased to oppose my idealism to his cynicism. The reader can judge for himself whether scientific objectivity is more compatible with this kind of idealism or with the cynicism of contemporary Marxists. After all, the United States has many defects—especially on matters of race—but its "idealism" does allow for change at the present time, whereas people believing in their hoped-for economic revolution can only look to the vague future to bring the millennium.

Arnold M. Rose

Department of Sociology,
University of Minnesota.
Dec. 21.

[1] *Nature*, **168**, 862 (1951).

Prejudice, like geraniums or sea-squirts, can only be effectively studied historically, developmentally and comparatively. It may be that this is 'economic determinism', though it used to be called 'the scientific outlook'; and, whatever it is, it precludes consideration of the mutational origin of prejudice in the nineteenth century. Moreover, when this unitary approach is controlled by application and the basic postulates of science proposed by Bertrand Russell (1948), an inclusive understanding emerges of which Dr. Rose is apparently not aware.

I regret I did not observe that Prof. Rose's pamphlet provides a "summary . . . of all the present-day theories advanced to explain prejudice". This insensitiveness was probably conditioned by ignorance of any comparable feat during three decades of association with scientific research.

I must avoid answering the rest of the above letter, because I find it either incomprehensible (items 1, 3, 4, 8) or distorting (items 2, 5, 7). The best guide to Dr. Rose's thought is his final comment. Many eminent scholars in Britain and elsewhere who happen to be Marxists, and many others who disregard Marxism but dislike social immaturity, will note his irrelevant conclusion.

Cedric Dover

"Design and Fitting of Laboratories"

The secretary of the Institute of Physics has pointed out to me an error in my review of "Laboratory Design" published in *Nature* of February 16, p. 256. I said that no designers spoke at the symposium of the Institute of Physics on the design of laboratories. In fact, two architects contributed to the discussion, which was reported in the *Journal of Scientific Instruments* in May 1948. The Institute is thus cleared of the implied charge of failure to appreciate the function of the architect; but much more could be done to bring home to both scientific workers and architects the value and help that each profession could be to the other. A first step has been taken to this end by the setting up, under the auspices of the Royal Institute of British Architects, of a joint committee to consider problems of laboratory design. This committee agreed that an up-to-date reference book is needed to cover the design of industrial, educational and research laboratories; but it was unable to solve the problem of financing such a project, without external aid from the Government or other appropriate bodies, which so far has not been forthcoming.

Stanley Meyrick

6 Fordwich Road,
Welwyn Garden City,
Hertfordshire.

FORTHCOMING EVENTS

Tuesday, April 15—Friday, April 18

CHEMICAL SOCIETY, in conjunction with the ROYAL INSTITUTE OF CHEMISTRY (in Dublin).—Anniversary Meetings.

Wednesday, April 16

At 11.30 a.m. (in the Lecture Theatre, Chemistry Department, Trinity College).—Sir Eric Rideal, F.R.S.: "Colloid Science—a Chapter in Chemistry" (Chemical Society Presidential Address).

Thursday, April 17

At 11.45 a.m. (in the Rupert Guinness Hall, Watling Street).—Dr. V. C. Barry: "Studies in the Chemotherapy of Tuberculosis".

Friday, April 18

At 11.30 a.m. (in the Lecture Theatre, Chemistry Department, University College Science Buildings, Upper Merrion Street).—Mr. H. W. Cremer: "Worthy Ends and Expectations" (Royal Institute of Chemistry Presidential Address).

Wednesday, April 16

BRITISH INSTITUTION OF RADIO ENGINEERS, LONDON SECTION (at the London School of Hygiene and Tropical Medicine, Keppel Street, London, W.C.1), at 6.30 p.m.—Mr. E. M. Lee: "Current Radio Interference Problems".

Wednesday, April 16—Friday, April 18

INSTITUTE OF PHYSICS, STRESS ANALYSIS GROUP (at University College, Gower Street, London, W.C.1).—Sixth Annual Conference.

Thursday, April 17

SOCIETY OF CHEMICAL INDUSTRY, ROAD AND BUILDING MATERIALS GROUP (at the Institution of Structural Engineers, 11 Upper Belgrave Street, London, S.W.1), at 10 a.m.—Conference on "The Proceedings of the Building Research Congress".

SOCIETY OF CHEMICAL INDUSTRY, AGRICULTURE GROUP (joint meeting with the MICROBIOLOGY GROUP and the BRITISH SOCIETY OF SOIL SCIENCE, in the Chemistry Lecture Theatre, Royal College of Science, Imperial Institute Road, London, S.W.7), at 10.30 a.m.—Discussion on "Soil Nitrogen Status".

BRITISH SOCIETY FOR THE HISTORY OF SCIENCE (in the Science Museum, Exhibition Road, London, S.W.7), at 2.30 p.m.—Symposium on "Historical Scientific Contacts between East and West".

INSTITUTION OF MINING AND METALLURGY (at the Geological Society of London, Burlington House, Piccadilly, London, W.1), at 5 p.m.—Scientific Papers.

LONDON MATHEMATICAL SOCIETY (at the Royal Astronomical Society, Burlington House, Piccadilly, London, W.1), at 5 p.m.—Dr. K. A. Hirsch: "Infinite p-Groups".

BRITISH INSTITUTE OF RADIOLOGY (in the Reid-Knox Hall, 32 Welbeck Street, London, W.1), at 8 p.m.—Mr. W. E. Schall: "Comparisons of Medical and Industrial Radiography" (Thirty-first Silvanus Thompson Memorial Lecture).

Friday, April 18

ASSOCIATION OF APPLIED BIOLOGISTS (in the Mining Lecture Theatre, Imperial College of Science and Technology, Prince Consort Road, London, S.W.7), at 11 a.m.—Scientific Papers.

BIOCHEMICAL SOCIETY (at The University, Liverpool), at 11 a.m.—Scientific Papers.

INSTITUTE OF NAVIGATION (at the Royal Geographical Society, 1 Kensington Gore, London, S.W.7), at 5 p.m.—Capt. P. V. H. Weems: "Recent American Work on Astronomical Navigation".

INSTITUTION OF MECHANICAL ENGINEERS (at Storey's Gate, St. James's Park, London, S.W.1), at 5.30 p.m.—Dr. Robert T. Knapp: "Cavitation Mechanics and its relation to the Design of Hydraulic Equipment" (James Clayton Lecture).

MANCHESTER STATISTICAL SOCIETY (joint meeting of the STATISTICAL METHODS STUDY GROUP and the INDUSTRIAL STUDY GROUP, at the Albert Hall, Peter Street, Manchester), at 6 p.m.—Mr. G. Morton: "Use of Electronic Calculating Machines in Economic Research and Applied Economic Problems".

BRITISH INSTITUTE OF RADIOLOGY (in the Reid-Knox Hall, 32 Welbeck Street, London, W.1), at 6.30 p.m.—Prof. W. V. Mayneord: "Some Applications of Modern Physics to Diagnostic Problems".

INSTITUTE OF ECONOMIC ENGINEERING (at the Engineers' Club, Albert Square, Manchester), at 7 p.m.—Mr. P. D. Vincent: "The Measurement of Productivity".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

CHEMISTS (with honours degree in chemistry or A.R.I.C. and post-graduate analytical experience) in the Central Laboratory at County Hall (experience in paint technology an advantage in one), and at the Northern Outfall Works, Beckton, London, E.6 (knowledge of sewage treatment an advantage)—The Medical Officer of Health (PH/D1), London County Council, County Hall, Westminster Bridge, London, S.E.1 (April 19).

SENIOR SCIENTIFIC OFFICER or SCIENTIFIC OFFICER (a first- or second-class honours degree in science is essential, and agricultural experience an advantage) for research on mineral metabolism of animals—The Secretary, Rowett Research Institute, Bucksburn, Aberdeenshire (April 19).

HEAD (Grade I) (honours graduate, with R.I.C. qualifications, and teaching and industrial experience) OF THE DEPARTMENT OF SCIENCE, and an ASSISTANT (Grade A) FOR PHYSICS AND PHYSICAL CHEMISTRY, Birkenhead Technical College—The Director of Education, 63 Hamilton Square, Birkenhead (April 21).

JUNIOR SCIENTIST or SCIENTIFIC TECHNOLOGIST II in connexion with operational research in the Field Investigational Group of the Department of Scientific Control—The National Coal Board, Establishments (Personnel), Hobart House, Grosvenor Place, London, S.W.1, endorsed TT/446 (April 22).

SCIENTIFIC OFFICER (with a good honours degree in mathematics and, preferably, specialized in mathematical statistics or analysis) mainly for research into animal and plant populations—The Establishment Officer, Nature Conservancy, 91 Victoria Street, London, S.W.1 (April 28).

SUPERINTENDENT FOR THE ZOOLOGICAL GARDENS—The Hon. Secretary, Royal Zoological Society of Ireland, Dublin (April 30).

PRINCIPAL SCIENTIFIC OFFICER (with a first- or second-class honours degree in chemistry or physics) at the Ministry of Agriculture and Fisheries Veterinary Laboratory, Weybridge, to be responsible for the freeze-drying of *Br. abortus* vaccine, and co-operation in studies on mechanization of production flow, on problems of air-filtration and the practicability of deep culture of *Br. abortus* and *M. tuberculosis*—The Civil Service Commission, Scientific Branch, Trinidad House, Old Burlington Street, London, W.1, quoting S.4133/52 (May 1).

PROFESSOR IN THE PHYSICS AND ELECTRICAL ENGINEERING DEPARTMENT—The Director of Studies, Royal Naval College, Greenwich, London, S.E.10 (May 3).

READERSHIP IN CHEMISTRY at Charing Cross Hospital Medical School—The Academic Registrar, University of London, Senate House, London, W.C.1 (May 6).

READERSHIP IN PHYSICS at Charing Cross Hospital Medical School—The Academic Registrar, University of London, Senate House, London, W.C.1 (May 6).

LECTURER IN ENGINEERING MATHEMATICS (including pure and applied mathematics and mathematical physics)—The Secretary, The University, Aberdeen (May 7).

READERSHIP IN BIOLOGY at Charing Cross Hospital Medical School—The Academic Registrar, University of London, Senate House, London, W.C.1 (May 7).

PRINCIPAL SCIENTIFIC OFFICERS (Physicists) (with a first- or second-class honours degree in physics or engineering, a specialized knowledge of eddy current phenomena, audio and low-frequency techniques, and able to take charge of projects involving research and development) in the Royal Naval Scientific Service, at the Underwater Counter-measures and Weapons Establishment near Portsmouth (one post will be ultimately at Greenock), to lead a group of scientists engaged on the development of electromagnetic devices—The Civil Service Commission, Scientific Branch, Trinidad House, Old Burlington Street, London, W.1, quoting No. S.4132/52 (May 9).

LECTURER IN PHILOSOPHY (specially qualified to teach logic and, if possible, the philosophy of science), an ASSISTANT LECTURER IN GEOGRAPHY, an ASSISTANT LECTURER IN PHYSICS, a LECTURER and an ASSISTANT LECTURER IN BIOLOGY, ASSISTANT LECTURERS (2) and a RESEARCH DEMONSTRATOR IN GEOLOGY, and a LECTURER IN PSYCHOLOGY—The Registrar, University College of North Staffordshire, Keele, Staffs (May 10).

RESEARCH FELLOWSHIP IN GOLD COAST CULTURE AND HISTORY in the Department of Archæology, University College of the Gold Coast—The Secretary, Inter-University Council for Higher Education in the Colonies, 1 Gordon Square, London, W.C.1 (May 10).

CHAIR OF ENGINEERING, a LECTURER or ASSISTANT LECTURER IN MATHEMATICS (with special reference to the teaching of mathematics to engineering students), and a HEAD OF THE DEPARTMENT OF GEOLOGY (with the status of Professor or Senior Lecturer), at the University of Malaya, Singapore—The Secretary, Inter-University Council for Higher Education in the Colonies, 1 Gordon Square, London, W.C.1 (May 17).

ENGINEER TO THE TRENT RIVER BOARD—The Clerk, 206 Derby Road, Nottingham (May 17).

LECTURER or ASSISTANT LECTURER IN MATHEMATICAL STATISTICS, and a RESEARCH ASSISTANT IN NUMERICAL ANALYSIS—The Registrar, The University, Liverpool (May 17).

LECTURER IN PHYSICS, an ASSISTANT LECTURER IN CHEMISTRY, and an ASSISTANT LECTURER IN MATHEMATICS, at the University College of the West Indies—The Secretary, Inter-University Council for Higher Education in the Colonies, 1 Gordon Square, London, W.C.1 (May 17).

SENIOR AGRICULTURAL ECONOMIST in the Department of Economics (Bristol II Provincial Agricultural Economics Centre, Newton Abbot)—The Registrar, The University, Bristol 8 (May 17).

LECTURER IN PHARMACOLOGY AND THERAPEUTICS, a SENIOR LECTURER IN PHYSICS, and a READER IN PHILOSOPHY, at the University of Hong Kong—The Secretary, Association of Universities of the British Commonwealth, 5 Gordon Square, London, W.C.1 (May 31).

WOOL TEXTILE RESEARCH COUNCIL SCHOLARSHIPS (one in the Department of Technical Chemistry to further chemical investigations on the basis of wool dyeing, and the other in the Department of Textile Technology to further physical investigations on the migration of twist in woollen yarns)—The Secretary, Royal Technical College, Glasgow, C.1 (May 31).

DAVID BROWN SCHOLARSHIP IN AGRICULTURAL ENGINEERING—The Registrar, King's College, Newcastle-upon-Tyne (July 15).

DEVELOPMENT ENGINEERS at the Ministry of Supply Factory, Capenhurst, Chester, to assist in the construction and operation of prototype plants, involving mechanical and chemical engineering—The Ministry of Supply, Division of Atomic Energy (Production), Risley, Warrington, Lancs, quoting Ref. 316.

NON-MEDICAL BIOCHEMIST (basic grade) to work in the laboratories on routine determination of steroids—The Chief Administrative Officer, United Sheffield Hospitals, West Street, Sheffield 1.

PHYSICIST for the Regional Radio-therapy Services—The Secretary, Newcastle Regional Hospital Board, Osborne Road, Newcastle-upon-Tyne.

SCIENTIFIC ASSISTANT (graduate in physiology or zoology with special interest in endocrinology)—The Director, Clinical Research Unit, Three Counties Hospital, Arlesey, Beds.

SCIENTIFIC OFFICER (with a university degree, or an equivalent professional qualification, and experience in chemical engineering and fuel technology) under the East African Industrial Research Board for duties which include the carrying out of technological investigations and evaluation of economic potentialities—The Director of Recruitment (Colonial Service), Colonial Office, Sanctuary Buildings, Great Smith Street, London, S.W.1, quoting Ref. No. 27106/55/52.

SENIOR LECTURER IN ELECTRICAL ENGINEERING at Bradford Technical College—The Director of Education, Town Hall, Bradford.

STATISTICIAN (Scientific Officer or Senior Scientific Officer grade) to work on problems and methods in genetics and biological experimentation—The Secretary, John Innes Horticultural Institution, Bayfordbury, Hertford.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Philosophical Transactions of the Royal Society of London. Series B: Biological Sciences. No. 632, Vol. 236: Late-Glacial Deposits at Nazeing in the Lea Valley, North London. By Jean Allison, H. Godwin and S. H. Warren; with a Report on Small Mammalia, by M. A. C. Hinton. Pp. 169–240+plates 7–14. (London: Cambridge University Press, 1952.) 28*s*. [311

Parliamentary and Scientific Committee. Annual Report 1951. Pp. 20. (London: Parliamentary and Scientific Committee, 1952.) [311

Proceedings of the Royal Society of Edinburgh, Section B (Biology). Vol. 64, Part 3, No. 13: The History, Organization and Work of Moredun Institute Animal Diseases Research Association. By Dr. J. Russell Greig. Pp. 253–256+1 plate. 1*s*. Vol. 64, Part 3, No. 14: Rowett Research Institute. By Dr. D. P. Cuthbertson. Pp. 257–276+2 plates. 4*s*. 6*d*. Vol. 64, Part 3, No. 15: Quantitative Evolution, 20, Correlations in Rates of Diversification. By Prof. James Small. Pp. 277–291. 2*s*. 6*d*. Vol. 64, Part 3, No. 16: Vascular Architecture of the Rat Uterus. By Archibald Young. Pp. 292–311+5 plates. 6*s*. Vol. 64, Part 3, No. 17: Inheritance of Resistance to Blight (*Phytophthora infestans*) in Potatoes; Inter-Relationships of Genes and Strains. By Dr. William Black. Pp. 312–352. 7*s*. Vol. 64, Part 3, No. 18: The Spermatozoon of the Spider Beetle (*Ptinus tectus*) Boieldieu as seen by Light and Electron Microscopy. By Jozef Dlugosz and John W. Harrold. Pp. 353–366+2 plates. 3*s*. 6*d*. (Edinburgh and London: Oliver and Boyd, Ltd., 1952.) [311

Ministry of Fuel and Power. 28th Annual Report on Safety in Mines Research, 1949. Pp. 62+6 plates. (London: H.M. Stationery Office, 1952.) 2*s*. 6*d*. net. [42

University of Leeds. Annual Report of the Librarian, Session 1950–51. Pp. 12. (Leeds: The University, 1951.) [42

Report of the Conference on the Use of Radioactive Isotopes in Industry. Organised by the F.B.I. Industrial Research Committee and the Atomic Energy Research Establishment and held at the Institution of Electrical Engineers, 1 May 1951. Pp. 52+10 plates. (London: Federation of British Industries, 1952.) 2*s*. 6*d*. [42

Universities Federation for Animal Welfare. Twenty-fifth Annual Report, 1st October 1950 to 30th September 1951. Pp. 24. (London: Universities Federation for Animal Welfare, 1952.) [42

Department of Agriculture: Fisheries Branch. Report on the Sea and Inland Fisheries for the Year 1950, incorporating Statistics of the Capture of Salmon, Sea Trout and Eels. (Pr. 543.) Pp. 48. (Dublin: Stationery Office, 1951.) 1*s*. 6*d*. [42

Department of Scientific and Industrial Research. Report of the Geological Survey Board for the Year 1950. Pp. ii+28. (London: H.M. Stationery Office, 1951.) 1*s*. 3*d*. net. [52

University of Birmingham: Joint Standing Committee for Research. Twenty-second Annual Report (Session 1950–51). Pp. 102. (Birmingham: The University, 1952.) [52

Ministry of Agriculture and Fisheries. Fishery Investigations, Series 1, Vol. 5, No. 4: Pollution of the Humber; an Account of Surveys carried out during the Summer of 1949. Pp. 20. (London: H.M. Stationery Office, 1951.) 2*s*. net. [62

Other Countries

Bulletin of the Geological Survey of India. Series A, Economic Geology. No. 3: Report on Glass-making Sands in parts of Vindhya Pradesh, Uttar Pradesh and Rajasthan. By D. R. S. Mehta. Pp. iii+16+4 plates. 1 rupee; 1*s*. 6*d*. No. 4: Cement. By Dr. J. B. Auden. Pp. 48. 1.8 rupees; 2*s*. 3*d*. (Calcutta: Geological Survey of India, 1951.) [231

Records of the Geological Survey of India. Vol. 81, Part 3: The Mineral Production of the Indian Union during 1947. By Dr. W. D. West. Pp. 389–563. 3.12 rupees; 6*s*. Vol. 83, Part 1: General Report of the Geological Survey of India for the Year 1949. By Dr. W. D. West. Pp. 396. 8.6 rupees; 13*s*. 6*d*. (Calcutta: Geological Survey of India, 1950–1951.) [231

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